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Essays on Delegation and Social Norms

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Abstract

This thesis presents the results of my doctoral studies at Middlesex University London. It contains different papers related to Labour Markets and presents some results on production, delegation and dishonest behaviour. The first study compares how group size interacts with both constrained and un-constrained resource environments, finding that resource limitations diminish production over time and that all the groups learn with experience. The second explores the effects of incentives in dishonest behaviour in both the gain and loss domain finding that contrary to theoretical predictions, subjects do not cheat more when they are facing a loss. The third, studies the distributions derived from different delegation scenarios. We find that the distributions derived from optional delegation are more egalitarian than the ones made under compulsory delegation. Finally, I study gender differences in delegation finding that gender biases only arise in compulsory delegation, and not under endogenous delegation, and at an agent level.

A Maria Jesus y Miguel.

Declaration

I, Lara Ezquerro Guerra, declare that this thesis titled, Essays on Delegation and Social Norms and the work presented in it are my own. I confirm that:

- This work was done wholly or mainly while in candidature for a research degree at this University.
- Where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated.
- Where I have consulted the published work of others, this is always clearly attributed. Where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work.
- I have acknowledged all main sources of help.
- Where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself.

Signed:

A handwritten signature in dark ink, appearing to be 'L. Ezquerro' or similar, written in a cursive style.

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Chapter 1

Introduction

This thesis presents the results of my doctoral studies at Middlesex University London. The title of the thesis is “*Essays on Delegation and Social Norms*” and contains four papers. All the studies are related to Labour Markets and present some results in production, delegation and dishonest behaviour.

In chapter 2 , “*Size and Resources Limitations within Teams*” a joint work with Marie Wong, I study how the joint effect of labour and resources limitations affect productivity over time in a classroom experiment.

Team production has been extensively studied in the experimental literature. This literature mainly focuses on factors not related to team size or resources scarcity (i.e. gender, identity, incentives...). There are some studies where they vary team size and see its effect¹, however, none of this previous literature studies how resources limitations affect team production over time. Understanding how these factors work together would be beneficial to achieve more efficiency in organizations.

For this purpose I run a classroom experiment at Middlesex University London using 314 freshmen doing different Business School degrees. Subjects had to produce paper aeroplanes (a la Bergstrom and Miller, 1999) in groups

¹One experimental example is Brewer and Kramer (1986). They vary team size from 7 to 31 members to see its effect in cooperation. They observe that long run cooperation in larger groups is more complicated.

with 1 to 5 participants.² The task was performed in five rounds of three minutes each.³ Immediately after each round the final output was calculated in front of all the class and the mistakes pointed out to group members so that there was the possibility to learn from round to round. Subjects had to follow very specific instructions in order to build as many paper aeroplanes as possible. Any deviation from the instructions would render the final product as being unfit. Since the model aeroplane given to the participants had marker lines, we provide subjects with markers. These markers represented the capital or amount of resources of each group as the use of other markers was forbidden. The number of markers varies across groups, some groups received the same number of markers as players (e.g. 5 players and 5 markers) while others received fewer markers than players (e.g. 5 players and 1 marker). We refer to this latter case as being resource constrained. Note, our interest is to compare how group size interacts with both constrained and un-constrained resource environments. Further we compared how the different sizes of resources scarcity affect production.

We find that resource limitations diminish production over time and that all the groups learn with experience. However highly constrained groups (e.g. 5 players and 1 marker) never overcome the coordination problem derived from their constraint. They are the ones producing less output per capita over time. By contrast, groups with small resources limitations (e.g. 5 players and 4 markers) are able to overcome their constraint through experience and coordination. These groups reach the same production levels as unconstrained groups. In addition, we use entropy (Shannon, 1948; Masisi et al., 2008) to measure the degree of diversity of the system⁴ and find that higher entropy is achieved for least resource constrained groups. This confirms that highly constrained groups do not achieve their maximal output potential over time.

We therefore conclude that substituting capital with labour is only possible through experience and when the difference between labour and resource

²Participants were randomly sorted into different groups according to a predetermined protocol.

³A non-recorded training period of four minutes was done before starting the official rounds. This ensured that students knew how to perform the task.

⁴In our case the degree of diversity could be measured using their abilities, or skills, at producing aeroplanes and how they evolve between rounds 1 and 5.

constraint is small.

The third chapter, “*Cheating and Money Manipulation*” is joint work with Gary Charness, Celia Blanco and Ismael Rodriguez-Lara. In this chapter we study the effects of incentives in dishonest behaviour in both the gain and loss domain when subjects reveal a piece of private information. Revealing this (truthful) information does not cause externalities as it only varies subjects own payoffs. We also study the difference in behaviour under different type of incentives when subjects manipulate their own earnings or do not manipulate them.

Cheating behavior may have noteworthy economic consequences. Take, for example, the Madoff’s case, the accounting fraud in Lehman Brothers, or the recent emission scandal at Volkswagen. The tax and customs authority in the UK (HM Revenue & Customs) estimates that nearly half of the tax gap (i.e., the difference between tax collected and what it ought to be collected) is due to under-declaration of income.

This chapter is related to the theory of incentives which rests on the idea that monetary incentives can be used to alter individual behavior. In addition, the research question is closely related to studies finding that loss contracts (i.e. up-front bonuses that workers can lose) increase workplace productivity. One example of this can be found in Fryer et al. (2012), where students whose teachers received a bonus at the end of the year performed worse than students whose teachers were paid in advance (and need to return some or all the bonus if their students did not meet the performance targets). The authors argue that upfront incentives can be used to trigger teachers’ loss aversion and improve students’ performance as a result. Arguably, their study is not designated to detect teachers’ cheating behavior. Our design provides a clean environment to test cheating behavior under loss aversion, when the results of cheating do not refer to performance or affect any third party.

We use a variant of the design developed by Fischbacher and Föllmi-Heusi (2013) where the task consist in rolling a ten sided dice and reporting its outcome. The dice rolling is done in private and we cannot observe if a particular individual cheats. However statistical tests on the aggregate data

show the extent to which the experimental population distorts the truth.

We have three treatments. In the Baseline treatment subjects receive a fixed amount for reporting a number. This treatment studies if subject do not lie in the absence of incentives to do so. We then proceed to examine behavior in the loss and the gain domains. Subjects are given their earnings in a closed envelope at the end of the session. We have two variations upon the baseline, the gain and the loss treatments. In the gain treatment, the reported number obtained from the toss of a die, determines the amount to be placed (by the experimenter) into the envelope, while in the loss treatment all envelopes contain the maximum possible earnings and an amount dependent on the reported number is subtracted. Using the predictions of Prospect Theory (Kahneman and Tversky, 1979) loss aversion would predict more cheating with the loss framing, since giving up money would seem to be more unpleasant than simply not receiving money.⁵ Having observed these results, we also implemented two treatments where the participants were able to physically manipulate their earnings by taking their earnings from an envelope (gain treatment) or putting money into an envelope after having received the maximum possible payoff at the beginning of the session (loss treatment). We expected less cheating in both money manipulation treatments as trusting subjects with the maximum potential payoff could increase the moral cost of cheating.

We find that in most cases the presence of incentives increase cheating behaviour. In the standard treatments where participants simply receive their payoffs in an envelope, we do not find evidence of more cheating with a loss frame than in the gain treatment. This is surprising since we were expecting more cheating in the loss scenario. This may be due to the fact that the sense of ownership over the endowment obtained in advance (loss treatment) might have been too weak in this design. We then implemented treatments in which the participants either took their earnings from an envelope (gain treatment) or put money into an envelope after having received the maximum possible payoff at the beginning of the session (loss treatment). Indeed, requiring the participants to engage in money manipulation led to

⁵Cameron and Miller (2009) and Groulleau et al. (2016) also found evidence of loss aversion rendering more dishonesty in a real effort task where subjects could lie about their performance.

less cheating specially in the loss treatment. In fact, there is no significant difference between behavior in the baseline treatment and in the loss treatment with money manipulation.

Our results are quite surprising and we interpret them as arising due to differences in beliefs and moral costs across treatments. Subjects in the money-manipulation loss treatment were more likely to feel that they had been trusted with the full potential payoff in the beginning and had different beliefs about the beliefs of the experimenter than people in the corresponding gain treatment. The different moral costs and beliefs are likely to be an important determinant in deciding whether (and by how much) to cheat. Also, note that the decision to return money in the loss frame could be seen as warm-glow giving (Andreoni, 1989, 1990).

In chapter four, “*Distributional Consequences of Endogenous and Compulsory Delegation*”, joint with Praveen Kujal we study the effect of endogenous and exogenous delegation in a hierarchical structure.

Assigning responsibilities to other people is necessary to carry out activities in big organizations. Delegation has two main implications. First, it allows the appointed agent to acquire new skills, expand their knowledge and competence. This mechanism could be crucial in shaping the hierarchical structure of the firm as the successful agent selected today will be the future manager. Second, it includes another decision which makes difficult to know who is ultimately responsible for the decision originating the final outcome.

Hamman et al. (2010) investigate how subjects behave when delegation is compulsorily introduced in a hierarchical structure where agents come from a competitive market. The principal has to delegate to an agent how to divide an endowment of £10 between the principal and the recipient. They find that allocation decisions are less fair under compulsory delegation relative to when there is no delegation option (standard dictator game). Principals select the agent that redistributes lower amounts to recipients (and more to principals) more often.⁶

⁶Hamman et al. (2010) also carry out some rounds of endogenous delegation. However

The experiment had 3 treatments. In the first treatment, a standard dictator game (Forsythe et al. 1994) where a principal divides £10 between himself an unknown recipient. In the second treatment, principals chooses between delegating to an agent or taking the decision themselves. In the third treatment we replicate the main results from Hamman et al. (2010).⁷

We find that under compulsory delegation distributions of principals and recipients are more different than in a standard dictator game (Hamman et al. 2010; Bottino et al. 2016). However, this is not the case when delegation is optional. That is, when principals can decide between delegating the decision to an agent or deciding themselves more egalitarian distributions occur. Moreover, optional delegation gives similar allocation outcomes, relative to compulsory delegation, as those observed in a dictator game. Agents behaviour change from compulsory to endogenous delegation as they redistribute higher amounts when delegation is optional. We run an additional endogenous delegation treatment where agents did not know that principals could decide not to delegate (informationally closer to compulsory delegation) and find no differences in results with the standard endogenous delegation treatment. Informational asymmetries could not explain differences in agents behaviour.

We partly reconcile our results using social distance (Hoffman et al. 1996) and responsibility (Charness, 2000). In this framework increasing social distance, between principal and recipient, lowers the amount allocated in a standard dictator game and diminishes the feeling of responsibility over the unfair outcome. In our experiment compulsory delegation imposes the highest social distance as the responsibility of the decision is forcibly passed on to the agent. This resulting in redistributing lower amounts. Meanwhile under endogenous delegation, the distance is chosen by the principal who can decide to delegate or not. As a consequence of a lower social distance, the average amounts redistributed to recipients are higher than in compulsory delegation. Still, this theories fail at explaining differences in agents

this rounds are only four and preceded by eight compulsory delegation rounds.

⁷The dictator game with agent environment has also been used to study the effect of agents or principals being punished by their actions (Coffman, 2011; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013).

behaviour.

We know from previous literature that male and females have different risk, social and competition preferences (see Croson and Gneezy, 2003, for a review). Moreover, there is evidence of a gender gap in labour markets where females reach top level positions less often than males (Gregory-Smith et al. 2014; De Paola et al. 2016) and earn less than their male counterparts even when doing the same type of work (Booth et al. 2003; Kulich et al. 2011). One potential gender bias generator in labour markets could be delegation as it shapes the hierarchy of organizations. In chapter five we study if male and female principals behave similarly when they can delegate. Further, do male and female agents make the same allocation decisions when delegation is compulsory and optional? Chapter five tries to answer these two questions. This chapter, “*Gender biases in Endogenous and Compulsory Delegation*”, is joint with Praveen Kujal.

The only paper studying the link between gender and delegation is Bottino et al. (2016). They use the dictator game with delegation (Hamman et al. 2010) finding that under compulsory delegation male and female principals behave similarly and appoint the agent who gives less to recipients. This results in both male and female principals delegating more to male agents over time. Our aim is to see if these differences still arise when delegation is optional.⁸

The results indicate that there are no gender bias in principals behaviour. However, gender differences arise between agents. In compulsory delegation male agents are selected more often by both male and female principals due to them redistributing lower amounts to recipients. In endogenous delegation this tendency disappears and female agents are selected slightly more often than males. The main result is that agents in endogenous delegation stop competing by redistributing lower amounts resulting in no gender differences in agent selection compared to compulsory delegation.

⁸There were two agents per session, one male and one female.

To sum up, gender biases occur at the agent level and only when delegation is compulsory. Male agents redistribute lower amounts to recipients and as a result are selected more often by principals. This is not the case when delegation is endogenous where agents allocate higher amounts to recipients and gender biases in agent selection is not observed.

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Chapter 2

Size and Resources Limitations within Teams

Abstract

The impact of team composition, group size and rewards on productivity has been extensively examined in management, economics and the psychology literature. Yet little has been done to unravel the link between resources limitations, group size, productivity and work experience at the same time. We ran an experiment on aeroplane building (a la Bergstrom and Miller, 1999) with freshmen. In each session groups were randomly formed. The number of group members gives us the labour endowment while each group is also randomly assigned a certain amount of capital. We find that resource limitations diminish production over time. The greater is the disparity between group size and resource availability, more adverse is the effect on per capita output, especially when groups are larger. We also observe that whatever their limitation may be groups improve with experience. However, experience by itself is not enough to overcome the coordination problem that resource constrained groups face.

Introduction

How team size and resources constraint impact team production is essential for achieving efficiency in any organization. It is equally important to

understand how teams operate under varying resource constraints and how efficiency evolves as groups become more experienced in the task. If groups converge in efficiency with repeat experimentation, or whether resource constrained groups' per-capita output deteriorates is an important question for any organization. We find that when resources are scarce productivity diminishes, moreover the greater the difference between team size and resources limitation the lower the production. However experience always helps to improve production, especially when there is no resources limitation. When resources constraint is mild, learning could help to overcome the problem and to produce similarly as when resources are not constraint.

In this paper we use a between subjects design to study the following. Firstly, given resource constraints, we study how varying team size impacts team performance over time. Secondly, we study how changing the degree of resource constraints for a given group (size) impacts its performance. Further, if resources limitations are present then do smaller teams cope with them better than larger ones? Finally, we also study how team experience in the task makes any difference in terms of productivity irrespective of the size of the team. That is, is the negative effect of greater size dominated by learning when the resources are constrained?

This paper provides a bi-dimensional experimental setting to explore the effect of resources limitations on the group size heterogeneity problem. In our experiment subjects have to build paper aeroplanes (a la Bergstrom and Miller, 1999) in groups of different sizes ($L= 1, 2, 3, 4$ and 5). Subjects have to follow very specific instructions for aeroplane construction. Any deviation from the instructions would render the final product as being unfit. Since the model aeroplane given to the participants had marker lines, we provide subjects with markers, i.e. capital ($K=1, 2, 3, 4$ and 5). Subjects can only use these markers to draw the lines. Any completed aeroplane without the marker lines is deemed unfinished. However, the number of markers varies across groups. Some groups receive the same number of markers as players ($L=K$) while others receive fewer markers than players ($L>K$). We refer to this latter case as being resource constrained. Note, our interest is to compare how group size interacts with both constrained ($L>K$), where $R=(L-K)=1, 2, 3$ and 4 and un-constrained ($R=0$) resource environments.

Overall, we find that the size of the resources limitations has a negative impact on the production per capita of the group. Groups with small resources limitations learn to coordinate and are able to minimize the effect of resource restrictions over time. These groups end up reaching similar production levels as those groups with no resource limitations. However, groups with large resources limitations are unable to coordinate over time and consequently their production level is always lower than that of groups with no resources limitations.

Literature Review

A considerable body of literature has developed around team composition and production in terms of gender (Guillen and Marreiros, 2014; Invanova-Stenzel and Kübler, 2005), cooperation (Brewer and Kramer, 1986), incentives (Beersma et al., 2003; Nalbaltian and Schotter, 1997), group identity and information (Eckel and Grossman, 2005; Charness et al. 2007; Staats, 2012; Haas and Hansen, 2007) and experience (Fang, 2012; Reagans et al. 2005).

The effect of gender on team production has been analysed among others, by Guillen and Marreiros (2014) and Invanova-Stenzel and Kübler (2005). Guillen and Marreiros (2014), study how the gender composition of pairs affects their outcome in a non-incentivised essay writing task. Females are found to perform equally but better than male teams. By contrast, Invanova-Stenzel and Kübler (2005) ask individuals to perform a memory game under two different incentive schemes: a competitive environment in which two pairs play against each other and a revenue sharing treatment where earnings are shared by team members. They find that men perform better than females within mixed pairs in a competition environment. In addition, women, perform better in an entirely female team when matched against a male team. However, both papers are limited to the study of pairs.

Cooperative attitudes of teams has been studied by Brewer and Kramer, (1986). They vary group sizes, making teams of 7 or 31 subjects, to understand the cooperative behaviour of subjects that play a public goods game.

They observe that large groups of 31 subjects contribute less in a public goods game than groups of 7 subjects, finding that cooperation in larger groups over time is more complicated.

The effect of incentives on team production has been studied using fixed sized groups by Beersma et al. (2003) and Nalbaltian and Schotter (1997). Beersma et al. (2003) find that a competitive reward structure improves speed and diminishes free riding, while cooperative rewards promote accuracy in teams of four subjects. Nalbaltian and Schotter (1997) study team production using groups of six people over various periods. They modify incentives and the nature of the supervision (change the probability of being caught shirking) but keep the number of team members constant and equal six in all the cases. They find that relative performance schemes are the most effective and that monitoring works when it is efficient.

Other studies show the impact of identity and information on teams. Eckel and Grossman (2005), explore how building team identity affects the outcome of a public goods game of five players. They find that team identity can help to overcome the free-riding problem and thus, improve team production. However, they also point out that subjects do not feel identified with a group unless they are able to interact with other group members. Moreover, Charness et al. (2007) found that a stronger group identification enhances coordination and surprisingly decreases cooperation. Related to interaction between team members, Staats (2012) studies the effect of team familiarity on production and finds that direct knowledge of who you are working with increases production.¹

Finally, some researchers focused on understanding the positive impact of experience on group production. Fang (2012) developed a theoretical model and tested it empirically to show how groups improve their efficiency over time. Meanwhile, Reagans et al. (2005) explore the effect of the different components of experience on team performance. In addition, Gardner et al. (2012) show that resources integration capability boosts performance over time. They are the only ones taking into account team members' resources.

¹Haas and Hansen (2007) analyse data from questionnaires obtained from a management consulting firm and find that different information types affect production in different ways and are not substitutes as claimed by previous research findings.

Still these resources are not material and they base their conclusions on a survey done on two firms of the same sector.

Most empirical literature keeps the team size fixed and focus on other issues not related to resources limitations. Meanwhile some theoretical researchers in management and psychology suggest that teams are most effective when they have the minimum number of workers required to perform the group task and get the job done (Guzzo, 1988; Hackman, 1990; Guzzo and Shea, 1992; Hamilton et al. 2003). On the contrary, large teams have poorer processes manifested in the understanding of the teams' objective, in rendering high level of participation and in support for innovation.

This existing literature focuses on different aspects of team production and even study group size and its effect on production. However none of the previous literature focuses on studying the joined effect of varying team size and resources limitations.

The rest of the paper is organized as follows. Section 3 describes in detail the material and methods used to perform the experiment. Section 4 includes our main results. Section 5 presents the discussion.

Materials and methods

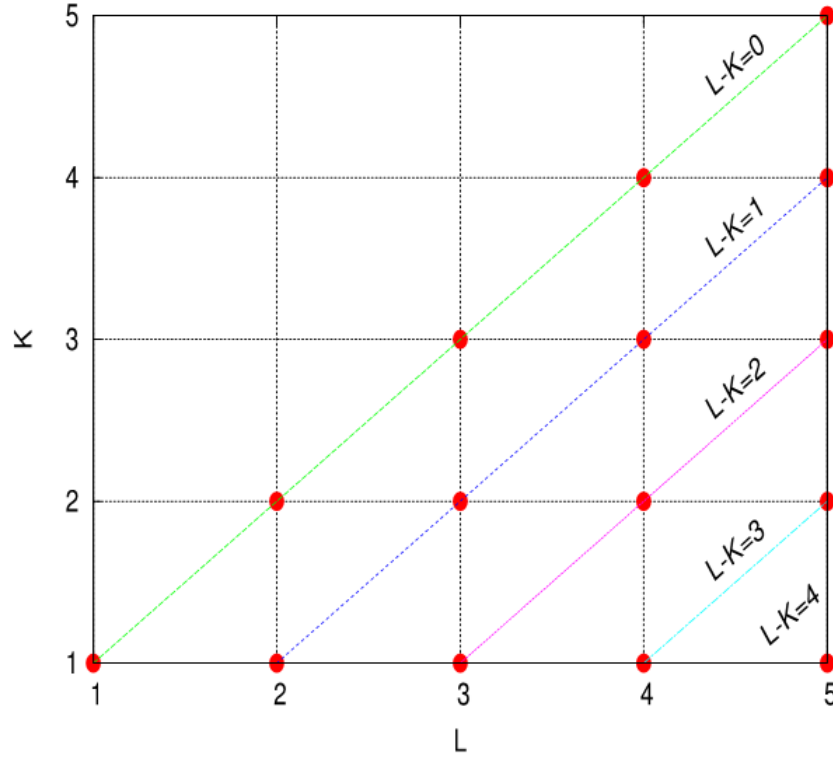
Experimental design

We vary the composition of teams with respect to group size (L) and resources constrain (K). Each possible combination of L and K will represent a different treatment. Figure 2.1 illustrates the total number of treatments (15 red dots) conducted. The horizontal axis reflects the number of workers (L) within the group while the vertical axis shows the number of resources -or markers- (K) provided to each group.

The groups in the main diagonal (green line) represent the five treatments without resource limitations since $R=0$ ($R=L-K$) in all the cases (i.e. $(1L, 1K)$, $(2L, 2K)$, etc.). The cases below the main diagonal represent

groups with resources limitations, i.e. $R > 0$. Notice that, the larger the distance (R) from the diagonal the higher the coordination problem faced by the team. For example, $(K=1, L=5)$, imposes the maximal resource constraint in our framework. Thus resource constraints increase from small, $R=1$ (dark blue dotted line), where the resource constraint is minimal to large, $R=4$, where the resource constraint on the team members is the largest.

Figure 2.1: Experimental Design



Implementation

The experiment consisted of 33 experimental sessions performed between November 2013 and October 2014 at Middlesex University London. Each of the sessions lasted for an hour and we followed a between subjects design.

The students were all freshmen taking different Business School degrees. Participants were required to do an activity that entailed a standard task designed by Bergstrom and Miller (1999). Participation was voluntary and no incentives were provided apart from the pure enjoyment of the activity and the competition encouraged between students in different groups. All participants we asked to sign an informed consent. Those who did not sign were allowed to leave the room.²

The allocation of students into groups was done randomly according to a predetermined (sorting) protocol. The protocol involved assigning the first person in the first column of the room to group 1, the second person to group 2 and so on until all the students had been allocated to a team and all the groups were complete. The randomization breaks any possible personal ties among participants as they were neither self-selected into groups nor in treatments.³

Once students were allocated into groups, they were provided with markers (K) and monitors explained that the activity involved constructing paper aeroplanes as shown on the instruction sheet (Appendix A contains the instructions of how to build the aeroplanes provided to students). Remark that under no resources limitations ($R=0$) there is no need to coordinate as subjects have one marker each. It is scarcity of resources what forces them to coordinate as they have fewer markers than team members ($R>0$). The experiment started with a four-minute practice round. The results of the practice session were not recorded. The practice session was followed by five rounds of aeroplane building activity with each round lasting three minutes. At the end of each round, we recorded the number of incomplete aeroplanes (we only accepted aeroplanes identical to the original model); the number of complete aeroplanes and those that were able to fly (we tested all the aeroplanes ability to fly in front of the crowd who applauded the successful aeroplanes). From now on we measure as output (Y) the number of functional aeroplanes produced by a group in each round.

²More than the 80% of the students asked to participate decided to do the activity and thus signed the informed consent.

³This protocol ensures a random assignment of students to treatments and minimizes any effect of team familiarity on production (Huckman et al. 2001; Easton and Rosenzweig, 2012; Staats, 2012).

Overview

Table 2.1 shows the basic statistics. The sample consisted of 314 students (148 females, 47.1%) randomly allocated into 87 groups. As mentioned above, these students were divided into teams of 1 to 5 participants who had access to different amounts of resources, also between 1 and 5 according to the different cases shown in Figure 2.1.⁴

Table 2.1: Descriptive statistics

	mean	min	max	observations
Group size	3.61	1	5	87 groups
Resources size	2.52	1	5	-
Female	47.1%	0	1	314 subjects

Results

Graphical Analysis

From now on we will focus on per capita output ($y=Y/L$). Figure 2.2 shows y as produced by every group in round 1. L is the number of people in each group; K is the number of markers per group. The output per capita ($y=Y/L$) of every group is shown in the vertical axis. In order to make the exposition clearer the vertical axis has been fixed in the median of all the groups (0.5 output per capita). Figure 2.2 illustrates that groups placed along the diagonal – except $(2L, 2K)$ – produce much more than the rest. In fact, those groups along $K=1$ notably underperform. By inspection,

⁴For every treatment (red dots in Figure 2.1) we have 6 independent observations at group level except for $(5L, 1K)$ and $(5L, 2K)$: 5 observations, $(5L, 5K)$: 4 observations - and $(4L, 4K)$: 7 observations.

it seems that the further away a group is from the diagonal the smaller is its per capita production. This reflects that when the team is inexperienced, having greater resources limitations diminishes the production of that team.

Figure 2.2: Per capita production in round 1

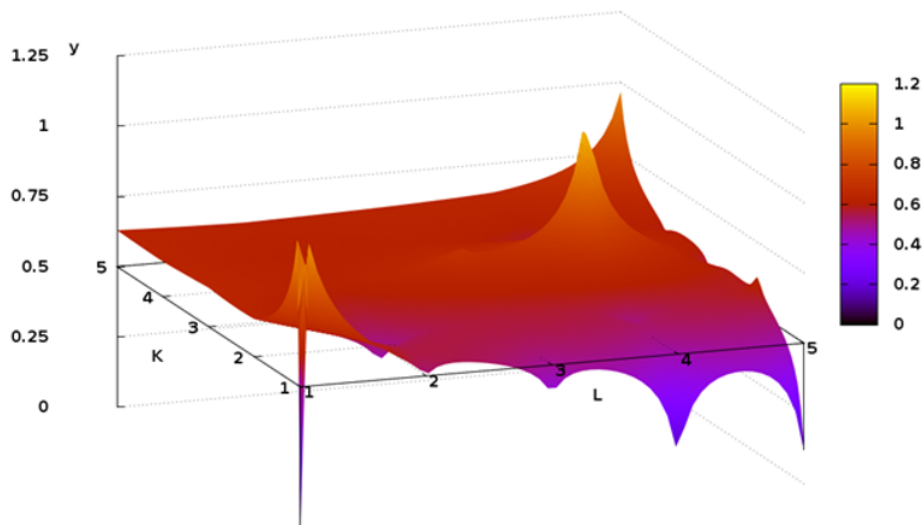
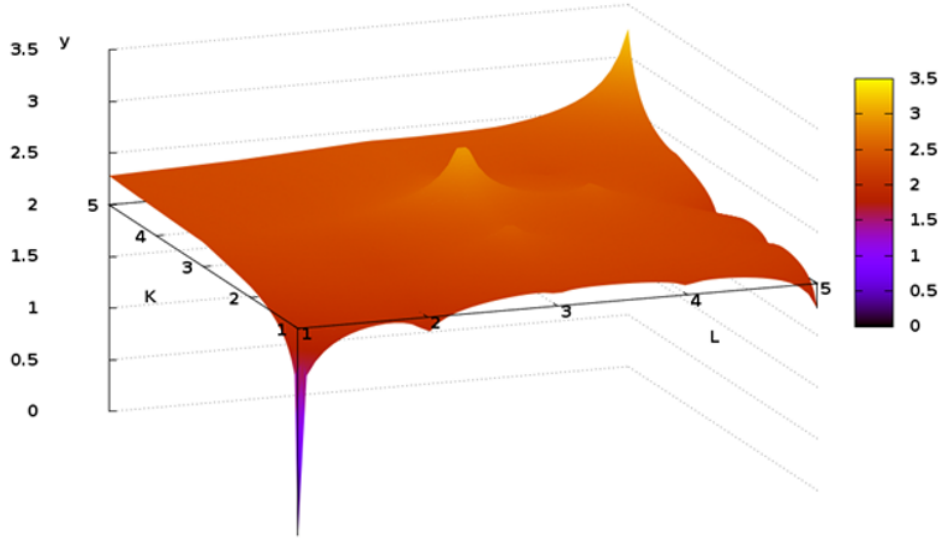


Figure 2.3, shows the output produced by every group in the last period, round 5. The vertical axis has been fixed in the median of all the groups (2 output per capita). In Figure 3, we observe that groups with small resources limitations ($R=1$ and $R=2$) adapt to the restrictions and produce almost the same as groups without restrictions. However, this is not true for groups with severe limitations ($R=3$ and $R=4$), even after several rounds they are not able to coordinate and produce as much as other groups with lesser limitations.

Figure 2.3: Per capita production in round 5



Statistical analysis

We estimate a double clustered OLS model where the dependent variable is per capita outcome ($y=Y/L$). The list of independent variables includes: the size of the groups, their access to resources and their experience in doing the task. We specify the model as,

$$y = \beta_0 + \beta_1 L + \beta_2 R + \beta_3 round + \varepsilon \quad (2.1)$$

where, β_1 measures the effect of team size, L , in per capita production. To capture the effect of the resources limitations we include the distance between group size and resources, R . This variable will go from no resources limitations (0 when $L=K$) to the maximum level of resource limitations (4 when $5L, 1K$). β_2 , will then measure the effect of increasing resource constraints in per capita production. β_3 accounts for the experience (round=1, 2, 3, 4, 5) while ε is the error term of our model. Table 2.2 summarizes the results for the previous model.

Table 2.2: Regression analysis

<i>MODEL</i>	<i>ALL THE GROUPS</i>		<i>GROUPS WHERE R>0</i>		<i>GROUPS WHERE R=0</i>	
	<i>I</i>	<i>II</i>	<i>III</i>	<i>IV</i>	<i>V</i>	<i>VI</i>
<i>L</i>	0.10 (0.407)	0.14 (0.286)	0.08 (0.527)	0.09 (0.493)	0.17 (0.357)	0.20 (0.329)
<i>R</i>	-0.21** (0.012)	-0.22*** (0.010)	-0.20* (0.068)	-0.19* (0.071)	-----	-----
<i>Round</i>	0.43*** (0.000)	0.44*** (0.000)	0.41*** (0.000)	0.41*** (0.000)	0.50*** (0.000)	0.50*** (0.000)
<i>Constant</i>	1.20 (0.140)	-0.02 (0.963)	0.75 (0.248)	0.16 (0.756)	-0.61 (0.908)	-0.43 (0.489)
<i>Control variables</i>	YES	NO	YES	NO	YES	NO
<i>Subjects</i>	308	308	206	206	102	102
<i>Number of groups</i>	81	81	58	58	23	23
<i>Prob > F</i>	0.00	0.00	0.00	0.00	0.00	0.00

Note: All the regressions were performed using a double clustered OLS. The clusters were performed according to groups and sessions. Since $L=K=1$ have only one single worker (not teams) we decided not to include them in the regression analysis. All the regressions were performed with and without controls for gender, ethnicity, residency outside UK, presence of the module's lecturer during the experiment and presence of a non-cooperative subject in the group. *, ** and *** indicate significance at $p=0.10$, 0.05 and 0.01 levels of confidence, respectively.

Model I and II listed in Table 2.2 show the estimated values of the model for all the groups with and without control variables (see notes in Table 2 for details). The results are similar in both models. The coefficient associated to L , is 0.10 and 0.14 respectively. In both models β_1 is not significant ($p=0.407$ in Model I and $p=0.286$ in Model II). This implies that the size of the group does not affect production per capita. Meanwhile, the coefficients associated to the resources limitations are negative (-0.21 and -0.22) and significant at a 5% level. The output per capita decreases when the access to resources is more limited. In addition, the coefficients associated to learning, are significant at a 1% level of confidence and positive (0.43 in Model I and 0.44 in Model II). The output per capita increases with the learning.

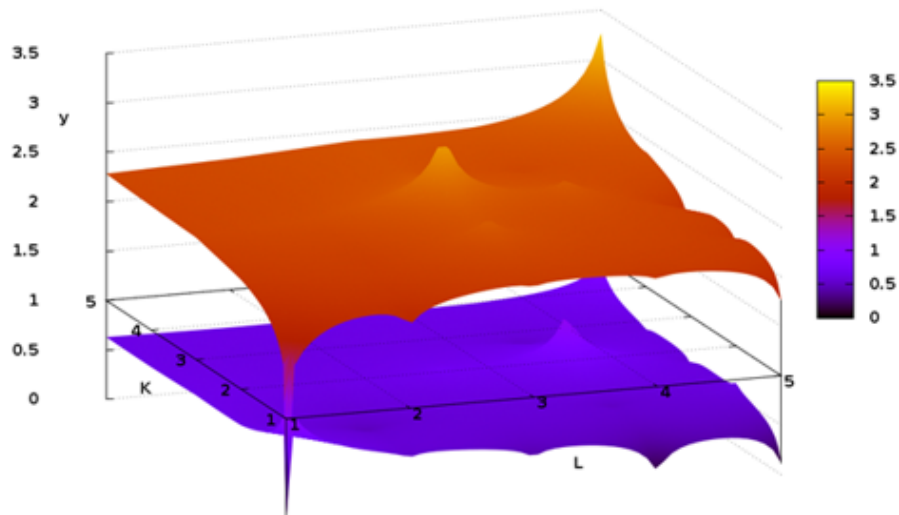
In Models III and IV we restrict the regression to those groups whose resources are limited ($R>0$). The results are similar with and without control variables. β_1 is not significant, the size of the teams is not relevant when the access to resources is limited. By contrast, the coefficient associated to the resources limitations, β_2 is negative and significant (10%) for both models (-0.20 for Model III and -0.19 in Model IV). The higher the resources limitations, the lower the output per capita. The coefficient associated to round is 0.41 and significant at a 1% level for both models. Experience has a positive impact on resource limited production.

Finally, in Models V and VI we isolate groups with no resources limitations, where $L=K$. Model V estimates the model with control variables and Model VI performs the regression without controls. As $L=K$, we will not have the variable R . Note that the coefficient β_1 is 0.17 in Model V and 0.20 in Model VI, the size of the group is again not significant at any level. Meanwhile, round is significant ($p=0.000$) and 0.50 in both regressions. This coefficient is higher than in Models III and IV where there were resource limitations. This hints that groups with unlimited access to resources learn faster as they focus on improving their building skills and do not have to learn how to coordinate. Also output per capita in groups with no resources limitations is not affected by the team size.

In conclusion, the size of the group is not relevant for per capita output when resource constraints are not binding. However, when resources are scarce, the distance between group size and amount of resources has a negative impact on output per capita. In addition, experience generates a learning process that increases the outcome. This learning effect is also higher when the access to resources is unconstrained, hinting that they learn faster when they do not need to coordinate. In sum, groups under extreme resource restrictions face serious coordination issues that are unable to overcome over time, while groups with small resources restrictions are able though experience to overcome coordination problems.

Figure 2.4 illustrates the learning process for the teams. L is the number of workers in each group; K is the number of markers in each group. Output per capita (y) is shown in the vertical axis. Note that the axis has been fixed to the average value of both rounds, 1. We can observe that almost all the groups increase per-capita output from round 1 to round 5. In spite of this, some groups learn faster than others. This is particularly the case where groups have almost no resources limitations. At the same time, when the resource constraints get increasingly severe, groups start to underperform even after the first period. This suggests that with large resources limitations, groups barely improve with learning as their coordination problem is too big to solve.

Figure 2.4: Per capita production in round 1 (in purple) and round 5 (in red and orange)



Entropy measures

Another way of analysing the bi-dimensional problem (size x resources limitations) proposed in this paper is the use of entropy. Entropy measures the degree of diversity of a system (Masisi et al. 2008). In our case the degree of diversity could be measured using their abilities, or skills, at producing aeroplanes and how they evolve between rounds 1 and 5. Each group is endowed with their own abilities and different capital endowments (markers). Using entropy we aim to measure how each group evolves in (per-capita) production and whether they achieve their maximum potential with time.

We calculate Shannon entropy (Shannon, 1948), defined by,

$$H = \sum_i p_i \ln p_i \quad (2.2)$$

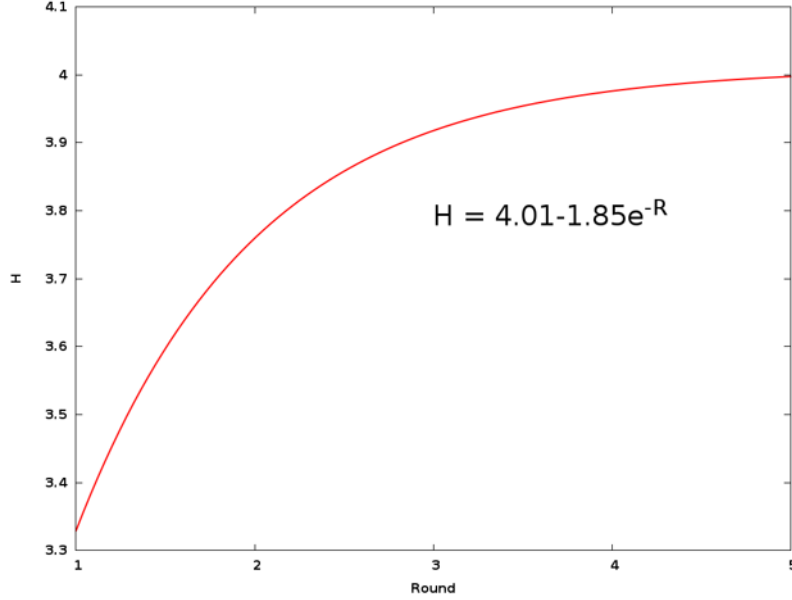
where p_i is the probability that a certain level of output, y , appears in groups of size L , with capital endowment K . The measure, H therefore allows us to measure per-capita output diversity in our experiment. Shannon entropy quantifies this diversity:

- The minimum value for H is zero, i.e. the per-capita output observed across all groups with the same resources and size is the same across periods.
- The maximal value of H occurs when each group produces (per-capita) output that is dissimilar.

Hence, a large value of H implies that each group produces a different level of per-capita output, hence resulting in maximum diversity (in per-capita) output. Therefore larger numbers result in greater levels of diversity, i.e. the probability that any level of per-capita output would appear.

Figure 2.5 shows the Shannon Entropy- H estimation for our data. The horizontal axis represents the round for which the entropy is shown on the graph. An important result, at aggregate level, is that entropy stabilizes with time. That is, with experience, groups converge in production per capita.

Figure 2.5: Shannon entropy measurement for rounds 1 to 5



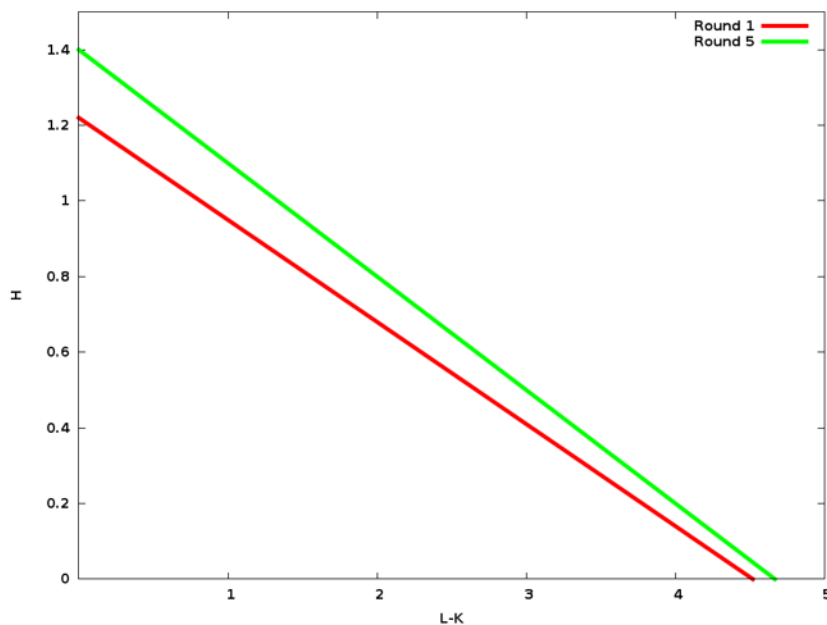
Now we look at per-capita production diversity at the group level. Note, we classify groups according to their resource constraints, i.e. $L-K=R$, $R = 0, 1, 2, 3, 4$. Recall, from Figure 2.1 that along the diagonal (i.e. $R=0$) groups are not resource constrained. However, as we move away from the diagonal they are, and the resource constraint becomes increasingly binding as we reach the maximal resource constraint at $R=4$. We first measure the per-capita diversity (as given by Shannon entropy measure) in each similar resource constrained group. We then approximate these points using an OLS linear regression for round 1 and, we repeat the process for round 5, where $H_t = b_t + a_t R_t$, where $t = 1, 5$. Figure 2.6 plots both regressions. The horizontal axis represents the difference between group size and their access to resources, R ($L-K$). We observe that there is a negative relationship between resource constraints and output per-capita. This is true for both rounds; 1 and 5. A negative gradient suggests that the diversity in production diminishes across groups as they get increasingly resource constrained.

Secondly, by comparing regressions for rounds 1 and 5 in Figure 2.6,

one observes that they tend to converge as the resource constraint, R , becomes increasingly binding. This suggests that groups with greater resources increase their diversity with time in terms of per-capita production. Meanwhile, highly resource constrained groups cannot achieve their optimal diversity with time, suggesting that resource constraints are a crucial element in the learning process related to production.

Note that, our results are similar to what is shown in the management and psychology literature (Guzzo, 1988; Hackman, 1990; Guzzo and Shea, 1992; Hamilton et al. 2003). They found that increasing group size is detrimental to performance. Meanwhile, we find that going off the diagonal moves groups away from achieving full entropy and therefore achieving their optimal way of producing.

Figure 2.6: Shannon Entropy-H: Linear Regressions for rounds 1 and 5



Discussion

We ran an aeroplane building task for undergraduate students. The students were not paid for the activity and their main motivation was inherent. The students were organized into different groups based upon a well-defined protocol. The research question we asked is how varying group size and resource constraints affect group productivity. We find clear evidence that substituting capital with labour is not possible over time. In fact, per-capita production suffers as the resource constraint becomes worse and there comes a point at which not even more coordination can help to overcome resources limitations.

We further use a measure of entropy to study how each group evolves with time. We find that no or low resource constrained groups ($R = 0, 1, 2$) perform better in the activity. Highly resource constrained groups ($R = 4, 5$), on the other hand, underperform as the size of the resource limitation increases. We find a negative relationship between the Shannon entropy measure and resource constraints. We achieve maximal entropy for the least resource constrained groups. This implies that they achieve maximum diversity in terms of per-capita output. Highly resource constrained groups do not achieve their maximal potential even after carrying out the activity repeatedly.

In terms of coordination, we find that if a group is not resource constrained then effectively group size does not matter. Our results points towards what could be an important oversight in the management literature when they look at group production and output. They argue that due to coordination problems larger groups are not effective. However we show that resources constraint plays a key role in explaining the relationship between team size and production. When resource constraints are trivial then effectively group size does not seem to matter as large and small groups produce similar amounts of per capita output. However when team size and available resources are increasingly divergent, even better coordination cannot help avoid the fall in per capita production.

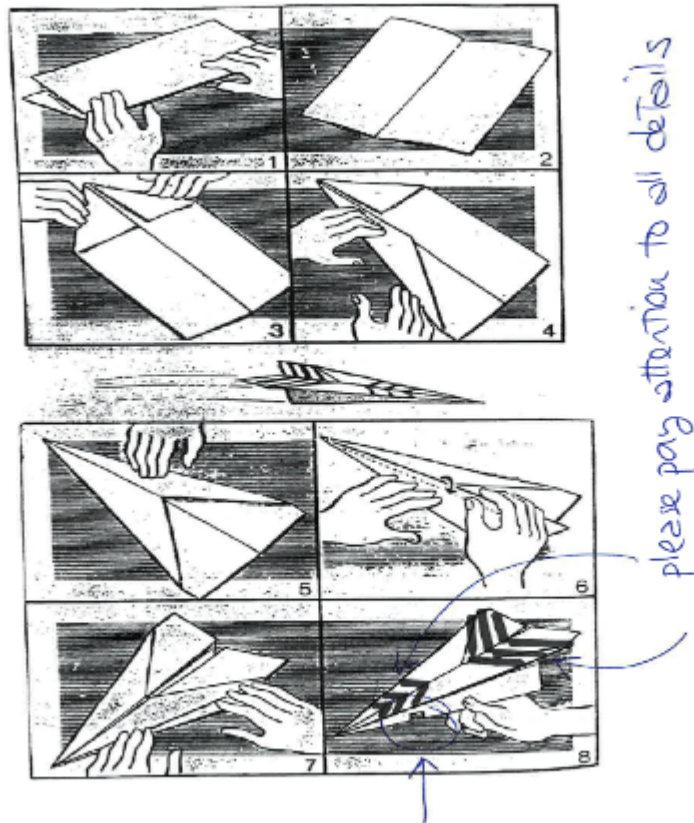
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Appendix A: Instructions

Groups should replicate the aeroplane in picture 8 as many times as possible in the given time.



Chapter 3

Cheating and money manipulation

Abstract

We use different incentive schemes to study how subjects cheat when they are asked to reveal a piece of private information. We find no significant evidence of cheating when there is no financial incentive associated with the reports, but cheating does occur when the reports determine financial gains or losses (in different treatments). No evidence of increased cheating is found to avoid a loss in the standard case in which subjects receive their earnings at the end of the session. When subjects manipulate the possible earnings, we find evidence of less cheating in the loss setting. We interpret our findings in terms of the moral cost of cheating and differences in the perceived trust and beliefs in the gain and the loss frames.

Introduction

Many economic interactions require that individuals disclose information that they possess. Examples include a car dealer selling a used car, a broker giving advice on the best mortgage, or a professor writing a reference letter for a student or a colleague. In all these environments with asymmetric information, it may be socially-optimal for individuals to reveal their information truthfully. However, economic and personal incentives may lead

people to deliberately misreport such information. One may shade the truth (so common in reference letters as to be the norm) or simply prevaricate. Such dishonesty is a form of cheating behavior, a term that also includes activities such as theft, embezzlement, and bribery.

There are noteworthy economic consequences associated with cheating behavior. Indeed, Cohn et al. (2014) point to cheating in the business culture as a force that is plausibly responsible (at least in part) for the all-too-common scandals in business and politics. Tax evasion and avoidance led to diminished tax revenue of approximately €1 trillion in the Eurozone, according to the European Commission, and the IRS estimates the overall tax gap in the U.S. represents about 16% of the estimated actual tax liability. In the developing world, corruption and cheating are quite prevalent; numerous dictators (e.g. Suharto, Marcos, and Duvalier) have shamelessly looted their countries, which have suffered greatly after the fall of the dictatorship; such corruption hinders investment and growth. In addition, recent experimental evidence has demonstrated that setting goals (Schweitzer et al. 2004) or using policies such as team incentives (Conrads et al. 2013), random bonuses (Gill et al. 2013), or performance-based bonuses (Jacob and Levitt, 2003; Martinelli et al. 2015) can exacerbate cheating behavior.

In this paper, we investigate the effects of incentives on cheating behavior in both the loss and gain domains when people are asked to reveal a piece of private information. This information concerns a state of the world, whose report only determines the payoff of the reporting agent. We focus on behavior when the money to be received is framed alternatively as a gain or a loss. There is evidence that loss contracts (i.e., up-front bonuses that workers can lose) increase workplace productivity (Brookset al. 2012; Hossain and List, 2012; Fryer et al. 2012). Further, workers might prefer loss contracts as a way to improve their performance and thus increase their expected earnings (Imas et al. 2016). However, Cameron and Miller (2009) and Grolleau et al. (2016) find that subjects cheat more in a loss frame when reporting their own performance on a real-effort task. Cameron et al. (2010) argue that paying people in advance for performing a task might induce a feeling of entitlement, and this might facilitate or even encourage unethical behavior.

In this paper, we investigate whether people cheat more in a loss frame when their private information concerns the state of the world. Since a loss contract usually requires that people receive the money in advance, we conducted treatments (in the gain and the loss frame) with and without money manipulation, as this may affect the “sense of ownership” and what could be termed the moral cost of lying. This device helps us to tease apart the effects of the frame and the manipulation of money on cheating behavior.

We consider an environment where the outcome is independent of one’s level of talent or ability, so that there should in principle be no measure of one’s worth attached to the report (of course considerations of self-image and social image may still affect the reports). We use a variant of the seminal design developed by Fischbacher and Föllmi-Heusi (2013). Each participant is asked to privately roll a die (6-sided in the original experiment, 10-sided in ours), so that the experimenter cannot determine the veracity of the subsequent report ¹.

The beauty of this design is that while the experimenter cannot know whether an individual is lying, statistical tests on the aggregate data show the extent to which the experimental population distorts the truth. Standard economic models predict that people will cheat in the absence of punishment when there is incentive to do so, but will otherwise be indifferent regarding telling the truth.

We first compare behavior in a Baseline treatment where subjects receive a fixed amount regardless of their report with the behavior of people when their financial payoff depends on the reported outcome. To our knowledge, this is the first paper that directly tests whether subjects lie in the absence of incentives when their behavior does not impose any payoff externality on others. We then proceed to examine behavior in the loss and the gain do-

¹We use a 10-sided die to increase the number of possible outcomes. Studies by, e.g., Hao and Houser (2010), Shalvi et al. (2011), Conrads et al. (2013), Gravert (2013), Jiang (2013) and Ploner and Regner (2013) have also used the die-rolling task. See Abeler et al. (2016) for a recent meta-study. Other studies have used the sender-receiver game where cheating is strategic (i.e., the sender needs to send a message to the receiver about the real state of the world and the receiver may believe it or not). This includes, among others, Gneezy (2005), Sutter (2009), Lundquist et al. (2009), Erat and Gneezy (2012), Erat (2013) and Vanberg (2015).

mains, since the notion of loss aversion (Kahneman and Tversky, 1979) is so pervasive. In our standard treatments, we give subjects their earnings in a closed envelope at the end of the session, but we change the reference point. In the gain setting, the reported number determines the amount to be placed (by the experimenter) into the envelope, while in the loss setting all envelopes contain the maximum possible earnings and we subtract an amount that depends on the reported number. Arguably, loss aversion would predict more cheating with the loss framing, since giving up money would seem to be more unpleasant than simply not receiving money.² Finally, having observed our results in these treatments, we implemented treatments in which the participants either took their earnings from an envelope (gain treatment) or put money into an envelope after having received the maximum possible payoff at the beginning of the session (loss treatment).

How would loss aversion actually apply in a cheating environment? As mentioned above, Cameron and Miller (2009), Cameron et al. (2010) and Grolleau et al. (2016) observe that loss aversion encourages cheating in real-effort tasks. Their tasks differ from ours in that one would expect more concern about one’s social image when one’s ability (or “honor”) is at stake. Previous experimental evidence (e.g., Ertac, 2011; Charness et al. 2014) shows that people are much less accurate in processing information when this information is self-relevant than when it concerns an outcome that is unaffected by one’s level of talent. Thus, we might expect more cheating in a self-relevant performance task than in our task (Gravert, 2013).

The closest paper to ours is Schindler and Pfattheicher (2017). They ask subjects to roll a 6-sided die 75 times and then report the number of ‘4s’ they have obtained. While subjects report more ‘4s’ in the loss frame, the authors find no evidence of cheating in the gain frame, contrary to other experimental evidence (Abeler et al. 2016). Another salient difference between our designs is that we ask subjects to report the outcome of a die roll, while the multiple die rolls in Schindler and Pfattheicher (2017) allows subjects to

²Garbarino et al. (2016) derive a prediction that people will lie more frequently when the probability of a (the) bad outcome is lower, since the higher expected payoff means that the “loss” avoided by lying compared to reference point is greater. They find support from an analysis of studies in the literature as well as new experiments. See also Abeler et al (2016) and Gneezy et al. (2016) for other experiments that vary the probability of a (the) bad outcome.

cheat more than once (see also Shalvi et al. 2011, Fischbacher and Föllmi-Heusi 2013).³ In addition, we complement their findings by looking at the moral costs and the effects of money manipulation on the reported outcomes.

We expect that moral concerns and social norms will interact with the motivation to cheat. With the mounting evidence on cheating behavior, some recent models include a term for the moral cost of lying but restrict this to be a function of the distance between the material payoff from cheating and that from not cheating (e.g., Lundquist et al.; 2009, Garbarino et al. 2016). However, it is likely that other elements should be present in the arguments of a function reflecting the moral cost of cheating. For example, Utikal and Fischbacher (2013) find that nuns tend to under-report the die roll, perhaps wishing to appear modest in their demands. Subjects also refrain from cheating when the opportunity is made salient (Mazar et al. 2008; Gino et al. 2009). The meta-study in Abeler et al. (2016) indeed concludes that the desire to appear honest may be a key driving force in explaining cheating behavior in the die-rolling task.⁴

In our setting, if you are given money and hold it in your possession, you may feel that you have been trusted (the mental-cheating condition could be seen as being a strong demand effect). Keeping the money with which you have been entrusted may feel more like stealing than taking money that you’ve been invited to take.⁵

Trust and morality are important and people may be sensitive to small clues and considerations (Mazar et al. 2008). If one feels trusted, this could mean that one believes that the trustor believes that the trustee will be-

³Schindler and Pfattheicher (2017) consider a second study in mTurk, where subjects self-report the outcome of tossing a coin (Buccioli, and Piovesan, 2011). In this task, where cheating is a binary decision, Schindler and Pfattheicher (2017) find that cheating occurs in both the gain and the loss frame, with more cheating being observed in the later.

⁴See also Rosenbaum et al. (2014), Kajackaite and Gneezy (2015), Gneezy et al. (2016), and Dufwenberg and Dufwenberg (2016) for related evidence, and Mazar et al. (2008) for a theory of self-concept maintenance.

⁵This resembles the idea of omission-commission in Spranca, Minsk and Baron (1991). However, participants in our experiment are asked to enter the number they have obtained in the computer screen, thus cheating requires acts of commission even in the loss condition (Cameron and Miller, 2009).

have in a trustworthy manner. If one doesn't, then one may experience guilt (Charness and Dufwenberg, 2006; Battigalli and Dufwenberg, 2007, 2009; Battigalli et al. 2013). We attempt to better understand the interplay between incentives to cheat in the gain and loss domain and the moral costs of cheating.

In fact, some of our experimental results will surprise many readers. We do find evidence across many of our treatments that cheating is more frequent when this affects the reporter's material payoffs than when it doesn't. However, in the standard treatments where participants simply receive their payoffs in an envelope, we find no evidence of more cheating with a loss frame than with a gain frame. We felt that two elements could have helped to induce this finding. First, reports are constrained to be one of the 10 possible outcomes of the die roll. If subjects cheat maximally in the gain treatment (given their moral costs), one shall not observe more cheating in the loss frame. Second, the sense of ownership might have been too weak in this design. We addressed these points by affecting the moral costs of cheating and implement treatments in which the participants actually physically handle the money. Indeed, requiring the participants to engage in money manipulation led to less cheating. To our surprise, however, we find substantially less cheating in this loss treatment than in the corresponding gain treatment. In fact, there is no significant difference between behavior in the baseline treatment and in the loss treatment with money manipulation.

Thus, we find ourselves swimming upstream with our experimental results. We do interpret our results as reflecting differences in perceived trust and beliefs. We suspect that there were different moral costs and beliefs in different treatments. Specifically, people in the money-manipulation loss treatment might have been more likely to feel that they had been trusted with the full potential payoff in the beginning and might have had different beliefs about the beliefs of the experimenter than people in the corresponding gain treatment. Further, the decision to return money in the loss framing could be seen as warm-glow giving (Andreoni, 1989, 1990), especially in the money-manipulation loss treatment, where subjects had to place the amount to be returned in an envelope. Hence, it seems unrealistic to ignore the psychological (moral) costs and benefits that are likely to be involved in deciding whether (and by how much) to cheat.

The remainder of the paper is organized as follows. We present the experimental design, implementation, and hypotheses in Section 2, and describe the experimental results in Section 3. We provide some discussion and conclude in Section 4 and 5.

Experimental design and hypotheses

Experimental design

A total of 426 subjects were recruited to participate in our experiment. We use the procedures in Fischbacher and Föllmi-Heusi (2013) and add our experiment at the end of a previous experiment that took around 90 minutes.⁶ All sessions were run at the Laboratory for Research in Experimental Economics (LINEEX) at the University of Valencia.

At the beginning of our experiment, subjects received a 10-sided die and a copy of the experimental instructions.⁷ Their task consisted of rolling the die privately in their cubicles and reporting the number from the first roll on the computer screen. Subjects could roll the die as many times as desired, but were told that only the first throw was relevant for their payment.

⁶Subjects in our experiment did not receive any feedback about the previous one until the end of the session. Our experiment was presented as an independent task to subjects, in which they could earn some additional money (all subjects decided to participate). This procedure is frequently used in the literature due to the short nature of the task.

⁷A translated version of the instructions can be found in Appendix B.

Table 3.1: Payoffs (in Euros) in each treatment depending on the reported number (0 to 9)

Number	0	1	2	3	4	5	6	7	8	9
Baseline	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5
Gain	€1	€1	€1.5	€2	€2.5	€3	€-3.5	€4	€4.5	€5
Loss	€-5	€-4	€-3.5	€-3	€-2.5	€-2	€-1.5	€-1	€-0.5	€0

We had five different treatments , which varied the payoffs that subjects received for reporting the outcome of the die (see Table 3.1) and the extent to which subjects manipulated their potential earnings.

- **Baseline treatment (Baseline):** At the end of the session, subjects received a sealed envelope with a fixed amount (€2.5), regardless of the number they reported.
- **Gain with no money manipulation (Gain-NO):** As in the baseline, subjects received their earnings in a sealed envelope at the end of the session. In this treatment, earnings ranged between €0 (when reporting 0) and €5 (when reporting 9).
- **Loss with no money manipulation (Loss-NO):** Before starting the session, subjects were informed that they had been allocated with an initial endowment of €5 to be kept in an envelope by the experimenter. Subjects were told that this would be used in a subsequent experiment. After finishing the first experiment (90 minutes), subjects were reminded about their €5 and presented with our task. Subjects knew that the reported number would determine the amount to be deducted from the envelope. This was given to subjects at the end of the session.

- **Gain with money manipulation (Gain-MM):** Again, earnings increased with the reported number but subjects had an envelope with €5 on their desk before rolling the die. Each subject had to extract their earnings from the envelope upon rolling the die and reporting the outcome on the computer screen.
- **Loss with money manipulation (Loss-MM):** Subjects received the initial endowment (€5) at the beginning of the session. They could keep this endowment on their table or their pockets. After finishing the first experiment (90 minutes), subjects were asked to take their initial endowment and were given an empty envelope. Subjects rolled the die, reported the outcome, and placed the amount to be returned in the envelope before leaving the room.

Before proceeding to the hypotheses, there are some aspects of our experimental design that are worth mentioning. First, earnings associated with each reported number were equivalent in the Gain and the Loss treatments. Second, we announced the initial endowment at the beginning of the session to subjects in the Loss treatments to trigger loss aversion. Finally, subjects in money-manipulation treatments had a second opportunity to cheat by misreporting the amount of money they had to take from or leave in the envelope. In this respect, our evidence is consistent with Cameron and Miller (2009) or Schindler and Pfattheicher (2017); we do not find that subjects recorded an outcome that did not correspond to the amount of money they took from or left into the envelope.

Hypotheses

Consider first the Baseline treatment. If people have standard preferences, we should expect reports to follow an equal distribution. We should also expect an equal distribution of reported numbers if lying has a cost. Even if one cares about social image, it is not obvious that rolling a higher number is better. Since we are not aware of any paper that directly tests for cheating behavior in the absence of economic incentives, our first prediction is:

Hypothesis 1: *The distribution of reports in the Baseline treatment is not significantly different from the uniform distribution.*

In all of our treatments except the Baseline, people have a financial incentive to cheat (Fischbacher and Föllmi-Heusi 2013, Shalvi et al. 2011, Abeler et al. 2016). If people value money and the cost of lying is not extreme, we should expect to see reports in the Gain and Loss treatments that are significantly higher than that from either the uniform distribution or the Baseline. Thus, our second hypothesis is:

Hypothesis 2: *The numbers reported in the Gain and Loss treatments will be significantly higher than those in the uniform distribution and in the Baseline.*

In line with the literature on loss aversion (Kahneman and Tversky, 1979) and a plausible link between loss aversion and choices made with gain and loss frames, we expected more cheating with loss framing, leading to our third hypothesis:

Hypothesis 3: *The numbers reported in a Loss treatment will be significantly higher than those reported in the corresponding Gain treatment.*

While cheating has clear financial benefits in the Loss and Gain treatments, it may also have a moral cost, e.g., subjects might be averse to cheat due to an intrinsic motivation to be honest (e.g., Lundquist et al., 2009), social image (the choice is observed by the experimenter) (e.g., Gneezy et al. 2016), a desire to hold a positive self view (e.g., Mazar et al. 2008), or some form of guilt aversion (e.g., Battagalli and Dufwenberg, 2007, 2009). A moral cost of cheating has been useful to explain why we observe truth-shading rather than universal reporting of either the true value or the maximum value in previous experiments. We therefore feel that entrusting people with money (on their desk or at the beginning of the session) will increase the moral cost of lying, leading to our final hypothesis⁸:

⁸One alternative, however, would be considering that there is an interplay between the type of incentives and the manipulation of money. One can thus argue that the manipulation can trigger the loss aversion, thus subjects can feel more entitled to keep the money they have manipulated in the Loss-MM. If that is the case we expect for the

Hypothesis 4: *The numbers reported in the money-manipulation treatments will be lower than in the corresponding treatments without money manipulation.*

Results

Figure 3.1 displays the distribution of the reported numbers in each of the treatments. Table 3.2 summarizes our data by including information on the frequency and cumulative distribution of reported numbers. The average reported number is above the mean expected outcome (predicted by the uniform distribution) of 4.5 in all the treatments. In fact, more high numbers (5-9) are reported than low numbers (0-4) in all treatments. Consistent with previous findings (Fischbacher and Föllmi-Heusi 2013, Utikal and Fischbacher 2013, Shalvi et al. 2011, Abeler et al. 2016), there is no single large spike at the payoff-maximizing outcome.⁹

outcomes to be higher in Loss-MM than Loss-NO.

⁹We also note that there is no significant gender difference in any of the five treatments. The overall average number reported by males (females) was 5.849 (5.633). The overall proportion of zeroes reported by males (females) was 0.050 (0.048), while the proportion of nines reported by males (females) was 0.171 (0.172). See, among others, Cappelen et al. (2012), Childs (2012), Gylfason et al. (2013) and Pascual-Ezama et al. (2015) for other studies showing no gender differences in cheating behavior.

Figure 3.1: Distribution of reported numbers per treatment

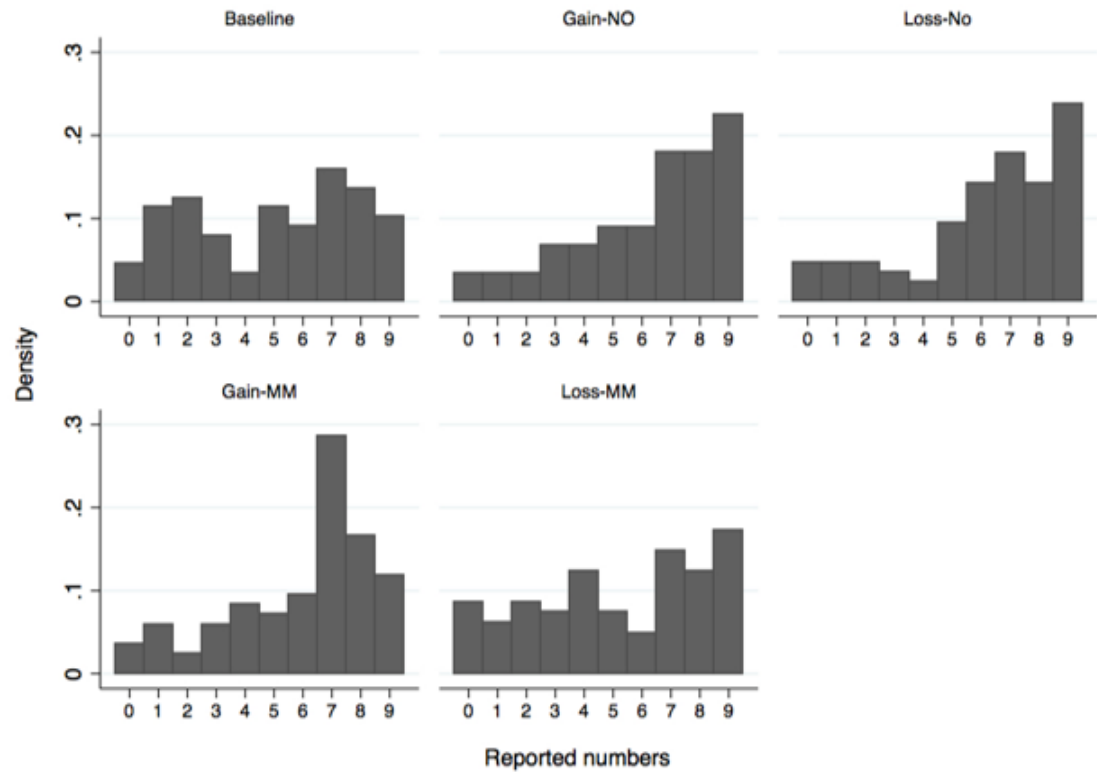


Table 3.2: Descriptive statistics: Frequency and cumulative distribution of reports

Tr	Obs.	Mean	SE	0	1	2	3	4	5	6	7	8	9
Base- line	88	4.977	0.304	4 (.046)	10 (.159)	11 (.284)	7 (.364)	3 (.398)	10 (.511)	8 (.602)	14 (.761)	12 (.898)	9 (1)
Gain NO	89	6.281	0.270	3 (.034)	3 (.067)	3 (.101)	6 (.168)	6 (.236)	8 (.326)	8 (.416)	16 (.596)	16 (.775)	20 (1)
Loss NO	84	6.214	0.291	4 (.048)	4 (.095)	4 (.143)	3 (.179)	2 (.202)	8 (.298)	12 (.440)	15 (.619)	12 (.761)	20 (1)
Gain MM	84	5.952	0.271	3 (.036)	5 (.095)	2 (.119)	5 (.179)	7 (.262)	6 (.333)	8 (.429)	24 (.714)	14 (.881)	10 (1)
Loss MM	81	5.198	0.332	7 (.086)	5 (.148)	7 (.235)	6 (.309)	10 (.432)	6 (.506)	4 (.556)	12 (.704)	10 (.827)	14 (1)

Note: Frequency is in whole numbers and cumulative distribution is in parentheses. SE = standard error.

We proceed to test our hypotheses with both non-parametric tests and regression analysis. First, the Kruskal-Wallis test rejects the hypothesis that observations in the different treatments come from the same distribution ($\chi^2 = 14.95$, $p = 0.005$), thus monetary incentives and/or the manipulation of money seems to affect the reported outcomes. The results of out non-parametric analysis are summarized in Table 3.3.¹⁰ We first investigate whether the reported outcomes in each of treatment differ from the actual expected outcomes (i.e., the equal distribution) using a χ^2 test, since the outcomes of the die rolling are numbers from 0 to 9. We also Wilcoxon rank-sum and Kolmogorov-Smirnov non-parametric tests to compare the reports in the Gain and Loss treatments with those in the Baseline (where subjects have no incentives to cheat).

¹⁰Throughout the paper, we round all p-values to three decimal places. The interested reader on the comparison between the reported outcomes in each treatment and expected actual outcomes using the Wilcoxon rank-sum test or the Kolmogorov-Smirnov test of cumulative distributions can consult Appendix A (Table 3.1). This includes information on the fraction of subjects who cheat to avoid the worst possible outcome using the estimation method in Garbarino et al. (2016).

Table 3.3: Non-parametric analysis on cheating behaviour

	Expected outcomes	Baseline outcomes	
Treatment	χ^2 test	Wilcoxon rank-sum (Z)	Kolmogorov-Smirnov (KS)
Baseline	12.00 (0.213)	-	-
Gain-NO	38.98 (0.000)	3.128 (0.001)	0.195 (0.024)
Loss-NO	39.57 (0.000)	2.880 (0.002)	0.213 (0.013)
Gain-MM	45.11 (0.000)	2.165 (0.015)	0.070 (0.039)
Loss-MM	11.72 (0.230)	0.610 (0.271)	0.185 (0.488)

Note: p-values (in parentheses) reflect one-tailed tests. The χ^2 test compares expected to actual outcomes.

Hypothesis 1 states that there will be no differences between reports in the Baseline treatment and the expected outcomes of the die roll. In fact, while we do see a slight tendency towards reporting higher numbers, the χ^2 test shows no significant difference between the reports in the Baseline and the actual expected outcomes ($p = 0.213$). Thus, we see no significant evidence of distortion in the reports made when there is no financial incentive for misreporting.

Result 1. *There is no evidence of cheating in the absence of economic incentives.*

We expected more cheating in our four treatments where there is a financial incentive to report a higher number than was actually rolled, as stated in Hypothesis 2. Indeed, the Gain-NO and Loss-NO treatments have much higher numbers than the expected true outcomes ($p = 0.001$) or the Baseline ($p = 0.024$). There is also evidence in the Gain-MM treatment of distortion relative to the expected outcomes ($p = 0.001$) and the Baseline ($p = 0.039$). However, there is surprisingly little difference between the reports made in the Loss-MM and Baseline treatments or between the reports in Loss-MM and the expected actual outcomes ($p = 0.230$).

Result 2. *Incentives affect cheating in all treatments except in the Loss-MM, where the distribution of reported numbers is very close to the expected actual outcomes and the Baseline distribution.*

Hypothesis 3 predicts that presumed loss aversion will manifest in more cheating in reports made in the loss frame than those made in the gain frame. As suggested in the preceding paragraph, the observed patterns do not support this hypothesis. In fact, there is very little difference in the reports across the Gain-NO and Loss-NO; the average report is in fact only slightly higher in the Gain-NO treatment (6.281 versus 6.214). The respective one-tailed test statistics and p-values are $Z = -0.089$, $p = 0.536$ and $KS = 0.042$, $p = 0.500$. What may be even more surprising is that there is substantially less cheating in Loss-MM than in Gain-MM. In any case, we have strong evidence to reject Hypothesis 3 in the money-manipulation treatments.

Result 3. *Incentives in the loss domain do not increase cheating behavior compared with incentives in the gain domain.*

Finally, Hypothesis 4 predicts that money manipulation will lead to less cheating due to an increased moral cost of lying. We find strong support for the hypothesis when we compare reports made in the Loss-MM and Loss-NO treatments. In fact, the median (modal) report in the Loss-MM treatment is 5 (7), while the median (modal) report in the Loss-NO treatment is 7 (9). The Wilcoxon test gives $Z = 2.128$, $p = 0.016$, while the Kolmogorov-Smirnov

test gives $KS = 0.230$, $p = 0.008$, both one-tailed tests. The differences across the reports in the Gain-MM and Gain-NO treatments are considerably more modest and not statistically significant. Here the Wilcoxon test gives $Z = 1.144$, $p = 0.126$, while the Kolmogorov test gives $KS = 0.119$, $p = 0.254$, both one-tailed tests. Thus, we see that money manipulation makes a real difference with a loss framing, but much less of a difference with a gain framing.

Result 4. *Money manipulation reduces cheating behavior, especially in the loss framing.*

We now proceed to the regression analysis. The model used is a Tobit since the sample is censored in the lower (0) and upper (9) limit. In Table we report the results of the Tobit analysis, where the set of independent variables include dummies for the gain frame and the manipulation of money, as well as the interaction term.¹¹ The reported standard errors (in parentheses) are clustered at the session level. Our first regression in column (1) uses the data from all treatments except the Baseline. Specifications (2) to (5) give the results for different Tobit models, depending on whether or not subjects manipulate the money and the frame.

As already suggested, there is no evidence of loss aversion in our data. If there is no money-manipulation, the behavior in the Loss frame is not significantly different from the behavior in the Gain frame. Money-manipulation does reduce the reported outcomes, but the effect is only significant in the loss frame, in fact subjects cheat more in the Gain than in the Loss treatment with money-manipulation.

¹¹In our Tobit analysis in Appendix A (Table 3.2) the set of independent variables include dummies for each of the treatment conditions, which are then compared with the Baseline reports. We note that ordinary least squares regressions provide qualitatively the same results and similar levels of significance. Our findings are also robust to controlling for the earnings of the previous experiment. While the subjects were not informed about such earnings when rolling the die, one might argue that they might had formed some beliefs to be used as a reference point. Table 3.1 in Appendix A presents the correlation between previous earnings and the reports.

Table 3.4: Regression Analysis

	(1) Pooled data	(2) No-Manipulation	(3) Manipulation	(4) Gains	(5) Looses
Constant	6.624*** (0.459)	6.628*** (0.480)	5.368*** (0.127)	6.636*** (0.231)	6.678*** (0.534)
Gain	0.061 (0.505)	0.061 (0.527)	0.757** (0.365)		
Money-Manipulation	-1.254*** (0.476)			-0.531 (0.386)	-1.285** (0.553)
Gain x Money-Manipulation	0.696 (0.605)				
Sigma	3.350*** (0.187)	3.376*** (0.197)	3.326*** (0.345)	3.043** (0.197)	3.685*** (0.192)
Num. observations (uncensored)	338 (257)	173 (126)	165 (131)	173 (173)	165 (120)
Pseudo LL	-762.751	-380.477	-382.262	-386.015	-374.673
Prob > F	0.076	0.901	0.157	0.265	0.031

Notes: Specification (1) includes data from all treatments except the Baseline. Specification (2) includes data from only the Gain-NO and Loss-NO treatments, while (3) includes data only from the Gain-MM and Loss-MM treatments. Specification (4) includes data only from the Gain-NO and Gain-MM treatments, (5) includes only data from the Loss-NO and Loss-MM treatments. ** and *** indicate significance at the $p = 0.05$ and $p = 0.01$ levels, respectively, two-tailed tests.

Discussion

Our results confirm some expected patterns. For example, people do not lie when there is no financial incentive to report an outcome different from the one that actually occurred; we find no significant difference between the reports made in this environment and the expected distribution of actual outcomes. We also observe considerable dishonesty when there is a financial incentive to report a higher number, which is consistent with previous work. Finally, money-manipulation seems to affect the moral cost of cheating and thus the reported outcomes.

But our other results are surprising. We expected to find evidence of more cheating when earnings associated to the report were framed as a loss, but we found absolutely no evidence of this in a standard environment. When we conducted treatments where the participants were given envelopes with the funds and then had to physically handle the money, we observe less cheating in the loss frame than in the gain frame! In fact, there was only a modest and insignificant difference in the reports in the Baseline treatment and the loss frame with money manipulation. This last result would appear to turn conventional wisdom on its head.

What could explain these unexpected results? An important consideration regarding loss aversion is the reference point for gains and losses and this may be unclear in laboratory settings (Terzi et al. 2016). While loss aversion and reference dependence are widely accepted, the generality of loss-aversion seems less than universal. A number of studies (e.g., Erev, Ert, and Yechiam, 2008; Harinck et al. 2007; Kermer et al. 2006) examining the effect of losses in decision-making under risk and uncertainty in fact find no evidence of loss aversion, as it occurs in our standard treatments. It is possible that subjects in our gain and loss treatments were already cheating maximally (given their moral costs), thus the (constrained) task prevented us to find more cheating under loss aversion; the spikes are in fact at the maximum value in the standard treatments.¹² We decided to affect the moral costs of

¹²Some people seem to care about reporting a higher number in the Baseline treatment, since more high numbers (5-9) are reported than low numbers in this case – more than 60% of the reports are high numbers. While there is no overall difference between the reports in the Baseline and the expected true values, it is nevertheless the case that this

cheating by asking subjects to manipulate the money. The spikes at higher numbers other than the maximum value in the money-manipulation treatments suggest that many people who choose to lie do not wish to either be seen as a liar (in fact by far the highest spike in all of the data is the spike at 7 in the Gain-MM treatment).

To our surprise, however, our null result was not driven by a lack of a sense of “ownership” of the funds as we find evidence of reversed loss aversion in the money-manipulation treatments. Harinck et al. (2007) document also this effect in a series of experiments where subjects are asked to rate how (un)pleasant would be finding (losing) small amounts of money. They argue that the negative feelings associated with small losses may be outweighed by the positive feelings associated with equivalent small gains. In our experiment, these feelings are likely to be affected by the damage to one’s self-image and the beliefs about what is expected. We suspect that if one feels trusted, one is more likely to respond in an honest or trustworthy manner.

It could be that it is more costly for subjects to cheat when they receive the money in advance because they feel they have been trusted. If the feeling of being trusted leads to different beliefs about what is expected, it could be the case that one believes that the trustor believes that one will behave in a trustworthy manner; otherwise, one may experience disutility from guilt.¹³ Perhaps people in the money-manipulation treatments had different beliefs about the expectations of the experimenter than people in the other variable-pay treatments. Having been endowed with visible money in the beginning of an experiment in the loss frame may also make cheating more salient than having an envelope from which people can later take money. Mazar et al. (2008) and Gino et al. (2009) find that subjects cheat less when cheating is salient. By asking subjects to return the money in the loss treatment, we might also trigger impure altruism (Andreoni, 1989, 1990).

60% is meaningful. The binomial test tells us the probability that 53 or more of 88 random draws in this treatment being high numbers is only 2.8%.

¹³This goes to the issue of whether one is more honest if one feels trusted. Some evidence is provided for this idea in Charness (2000). Additionally, Campbell (1935), May and Loyd (1993) and Haines et al. (1986) find that an honor system induces more honesty than does a proctor. See also Mazar et al. (2008) or Gino et al. (2009) for the related evidence on the importance of honor codes on cheating behavior.

One might nevertheless wonder why there was more cheating in the loss frame than in the gain frame in other studies. In our study, there is nothing regarding one’s own ability, while the work of Cameron and Miller (2009), Cameron et al. (2010) or Grolleau et al. (2016) involves reporting one’s own performance in a cognitive task. One’s judgement about own ability seems to be more malleable than one’s judgement about events over which one has no control, as seen in the updating studies mentioned earlier. Indeed, lying is likely to have some moral cost, but one would like to appear talented or capable in the eyes of those who may be watching or even one’s self; in fact, this may not even be a conscious tendency (see Charness et al. 2014). Using the die-roll task, Schindler and Pfattheicher (2017) allows for multiple rolls of the die and find that reports are larger under loss aversion, but the authors do not find evidence that people cheat in a gain setting. We complement their findings by also looking at the effects of the moral costs, which seems to be a crucial element in understanding cheating behavior.

Conclusion

Our paper investigates cheating behavior when experimental participants are asked to reveal a piece of private information that does not reflect on their personal ability and where one’s choice does not affect the financial payoffs of other participants. This information concerns the state of the world. In the Baseline treatment, there is no financial incentive for misreporting the state of the world and the reports made do not differ significantly from expected outcomes with random draws. On the other hand, reports when there are financial incentives to cheat generally show considerable evidence of lying on the reports made. In addition, we study cheating in the absence of payoff externalities, in that only one’s own material payoff is affected by reported outcome.

We do not find evidence that loss aversion translates into this environment. There is no difference in behavior across gain and loss frames when payment is simply made at the end of the session. More remarkably, when we endow participants with prospective payment in advance, there is substantially less cheating in the loss frame. We presume that the observed behavior

represents differences in the moral cost of cheating, reflected by either some form of guilt aversion or a desire to have a favorable self-image or social image.

Our results represent a challenge for the more standard behavioral theories such as loss aversion and reference points. It seems that the moral cost of behavior is an element that must not be ignored. We expect more research will follow on this theme, as it is critical to understanding cheating and corruption in the world at large.

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Appendix A

In the Results, we use a χ^2 test to investigate whether the reported outcomes differ from the expected ones (i.e., the equal distribution). In the first columns of Table 3.1 of this Appendix, we show that our results are robust to the Wilcoxon rank-sum test (Z) and the Kolmogorov-Smirnov (KS) test of cumulative distributions, except for the Loss-MM treatment, where the one-tailed comparisons between the reports and the expected actual outcomes come close to statistical significance.¹⁴

Table 3.1: Non-parametric analysis (Wilcoxon rank-sum and Kolmogorov-Smirnov tests), fraction of cheaters and correlation between reported outcomes and previous earnings in each treatment

Treatment	Z	KS	% cheaters	Correlation
Baseline	1.099 (0.272)	61.810 (0.405)		-0.125 (0.245)
Gain-NO	04.095*** (0.000)	14.056*** (0.000)	62.43%	-0.148 (0.802)
Loss-NO	3.886*** (0.000)	15.454*** (0.000)	48.37%	-0.028 (0.802)
Gain-MM	0 3.296*** (0.001)	12.448*** (0.001)	60.12%	-0.068 (0.541)
Loss-MM	1.563* (0.059)	3.459* (0.089)	21.99%	-0.004 (0.971)

Notes. * and *** indicate significance at the $p = 0.10$ and $p = 0.01$ levels, respectively. p-values are reported in brackets.

¹⁴We assume that the number of observations is 85 and perform one-tailed analysis, except for the Baseline treatment, where we consider a two-tailed hypothesis.

The literature on cheating behavior has usually identified honest subjects as those who report the worst possible outcome (Fischbacher and Föllmi-Heusi, 2013). The third column of Table 3.1 reports the fraction of subjects who cheat to avoid the worst possible outcome (receiving nothing) using the estimation method in Garbarino, Slonim and Villeval (2016). The last column of Table 3.1 reports the correlation between the reported outcomes and the participants' earnings in the previous experiment. While this is negative and never significant.

In Table 3.2, we report the results of a Tobit analysis, where the set of independent variables include dummies for our treatment conditions. The reported standard errors (in parentheses) are clustered at the session level.

Table 3.2: : Regression analysis (using the Baseline as the benchmark)

	(1) Pooled data
Constant	5.079*** (0.203)
Gain-NO	1.602*** (0.392)
Loss-NO	- 1.541*** (502)
Gain-MM	1.046*** (0.372)
Loss-MM	0.289 (0.241)
Num. observations (uncensored)	426 (332)
Pseudo LL	-937.023
Prob > F	0.004

,where the number in parenthesis represent the standard errors for each coefficient. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In line with our previous analysis, we find that all treatments are sta-

tistically different from the Baseline ($p < 0.01$) except for the Loss-MM treatment ($p = 0.231$). Pairwise comparisons confirm that there is no significant difference between reports in Gain-NO and Loss-NO ($p = 0.674$), while there is a significant difference between the reports in the Gain-MM and Loss-MM treatments ($p = 0.016$). We also see that there is a significant difference between the reports in the Loss-NO and Loss-MM treatments ($p = 0.003$), while the difference between the reports in the Gain-NO and Gain-MM treatments is weakly significant ($p = 0.062$), this suggesting that the manipulation of money can also have an effect in the Gain treatments in the expected direction.

Appendix B: Instructions

Instructions

The aim of this experiment is to study decision-making. We are not interested in your particular choices but rather on the individual's average behavior. Thus, all through the experiment you will be treated anonymously. Neither the experimenters nor the people in this room will ever know your particular choices. Please do not think that we expect a particular behavior from you. However, take into account that your decisions along the experiment may affect your earnings. Below, you will find the experimental instructions which detail how the experiment unfolds. Please follow them carefully, as it is important that you understand the experiment before starting. Talking with each other is forbidden during the experiment. If you have any questions, raise your hand and remain silent. You will be attended by the instructor as soon as possible.

What is the experiment about?

Baseline treatment:

Your task consists on throwing the 10 sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

Number	0	1	2	3	4	5	6	7	8	9
Amount	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5	€2.5

This means that you will earn €2.5 regardless of the number that you report.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, introduce this number in the computer screen.

You can throw the dice as many times as you want to test that it works properly. Still, your payment depends only on the number you report for the first throw.

At the end of the experiment, you will receive your earnings (in an anonymous way) in a sealed envelope.

Gain-NO treatment:

Your task consists on throwing the 10 sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

Number	0	1	2	3	4	5	6	7	8	9
Amount	€0	€1	€1.5	€2	€2.5	€3	€3.5	€4	€4.5	€5

This means that you will earn €0 if the number you report is 0, €1 if the number you report is 1, €1.5 if the number you report is 2, so on, obtaining an amount of €5 if you report a 9.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, introduce this number in the computer screen.

You can throw the dice as many times as you want to test that it works properly. Still, your payment depends only on the number you report for the first throw.

At the end of the experiment, you will receive your earnings (in an anonymous way) in a sealed envelope.

Loss-NO treatment:

Before starting the experiment you received €5. Your task consists on throwing the 10 sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

Number	0	1	2	3	4	5	6	7	8	9
Amount	€-5	€-4	€-3.5	€-3	€-2.5	€-2	€-1.5	€-1	€-0.5	€0

This means that you will return €5 if the number you report is 0, €4 if the number you report is 1, €3.5 if the number you report is 2, so on, returning an amount of €0 if you report a 9.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, introduce this number in the computer screen. We shall subtract the amount that you need to return from your initial €5. You can throw the dice as many times as you want to test that it works properly, still your payment depends only on the number you report for the first throw. At the end of the experiment, you will receive your earnings (in an anonymous way) in a sealed envelope.

Gain-MM treatment:

Your task consists on throwing the 10 sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

Number	0	1	2	3	4	5	6	7	8	9
Amount	€0	€1	€1.5	€2	€2.5	€3	€3.5	€4	€4.5	€5

This means that you will earn €0 if the number you report is 0, €1 if the number you report is 1, €1.5 if the number you report is 2, so on, obtaining an amount of €5 if you report a 9.

This means that you will earn €0 if the number you report is 0, €1 if the number you report is 1, €1.5 if the number you report is 2, so on, obtaining an amount of €5 if you report a 9.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, report this number using the computer screen. There is an envelope with €5 on your table. Take the money that corresponds to your throw and sealed it.

You can throw the dice as many times as you want to test that it works properly, still your payment depends only on the number you reported for the first throw.

At the end of the experiment, the instructor will pick up the sealed envelopes when you leave the room. Your earnings will be anonymous.

Loss-MM treatment:

Before starting the experiment you received €5.

Your task consists on throwing the 10 sided dice that you received memorizing the number that you obtain in the first throw. This number will determine your earnings as is shown in the table below.

Number	0	1	2	3	4	5	6	7	8	9
Amount	€-5	€-4	€-3.5	€-3	€-2.5	€-2	€-1.5	€-1	€-0.5	€0

This means that you will return €5 if the number you report is 0, €4 if the number you report is 1, €3.5 if the number you report is 2, so on, returning an amount of €0 if you report a 9.

First, we ask you to roll the dice and memorize the number you obtain in the first throw.

Then, introduce this number in the computer screen. Place the amount that you need to return in the envelope and sealed it.

You can throw the dice as many times as you want to test that it works properly, still your payment depends only on number obtained on the first throw.

At the end of the experiment, the instructor will pick up the envelopes. Your earnings will be anonymous.

Chapter 4

Distributional Consequences of Endogenous and Compulsory Delegation

Abstract

We study endogenous delegation in a dictator game where the principal can choose whether to delegate or not the allocation decision. Roughly half the subjects choose to delegate their decision. Our main result is that more egalitarian distributions occur when it is optional relative to when it is compulsory. This result holds both when principals and agents make the decision under endogenous delegation. When principals delegate, the allocation outcomes are similar to those observed under the standard dictator game and agents allocate more to recipients relative to principals. Finally, principals in compulsory delegation choose agents that allocate less to recipients, this relationship is weaker in endogenous delegation.

Introduction

Delegation is a useful tool in organizations for several reasons. It is argued that delegation is desirable for efficiency reasons, leader formation and also to pass decision making to outside sources. The experimental literature has studied delegation in different scenarios such as hierarchical structures

(Hamman et al. 2010; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013), bargaining scenarios (Ferhrstman and Gneezy, 2001) or labour markets environments (Charness et al. 2012). One limitation of this literature is that most studies do not take into account that delegated decisions are often made by the principal themselves and not exogenously imposed upon them.

In this paper we study the distributional consequences of endogenous delegation in a dictator game. We find that endogenous delegation generates more equal distributions than exogenously imposed delegation. Further, when principals make the allocation decision in endogenous delegation they transfer less to recipients (an average of 3.14) than when agents decide (3.83).

Delegation in hierarchical structures has been studied in experimental literature by Hamman et al. (2010), Bartling and Fischbacher (2012) and Oexl and Grossman (2013). The results from these experiments point towards delegation being used as a tool to hide behind unfair decisions. Hamman et al. (2010) find that under compulsory delegation the amounts redistributed to recipients are lower than in the standard dictator game. Hamman et al. (2010) also run experiments with endogenous delegation. Subjects first play 8 periods of compulsory delegation and then, 4 periods of endogenous delegation. They find that subjects in endogenous delegation behave similarly to subjects in compulsory delegation. We have also replicated this treatment and obtain the same results. It seems that once principals and agents become used to a lower social norm they carry it onto the endogenous delegation part. Subjects who first play compulsory and then endogenous delegation were affected by the previous periods and the learning they obtained from it. Meanwhile, Bartling and Fischbacher (2012) and Oexl and Grossman (2013) use a four player dictator game with one principal, one agent and two recipients, one of which will be able to punish the rest of the players after the division of the endowment has been made. Endowment can only be divided in two ways: unfair or fair. They find that delegation is effective at avoiding principals being held responsible for the unfair decision as recipients also punish agents for unfair decisions that have been delegated by the principal. The main results from this literature is that the distribution is less egalitarian when delegation is implemented. Overall, agents favor the principal's and transfer less to the recipients.

Hamman et al. (2010) studied exogenously imposed delegation upon the principals and found that principals select agents that maximize their payoffs and consequently reallocate lower amounts to recipients. By requiring compulsory delegation they remove principals from the decision to delegate or not. They find that when removed from this decision, principals select agents that maximize their payoffs. We ask the question what happens when delegation decisions are endogenous in the dictator game. We first replicate the results of Hamman et al. (2010). We obtain qualitatively the same results. Subjects redistribute *less* under compulsory delegation than under the standard dictator game. We then allow the delegation decision to be endogenous, where principals can choose to delegate the decision or decide themselves. A priori one would expect that those who delegate, i.e. ‘pass on the responsibility of the decision’, will behave in a selfish manner and choose agents who reallocate lower amounts to recipients (Hamman et al. 2010). The motives of those that choose not to delegate are not clear. It could be that they prefer to make the decision on their own for either the prosocial or selfish motive. We find that endogenous delegation always generates more egalitarian distributions relative to compulsory delegation. Surprisingly, the distribution made by agents under endogenous delegation converges to the one observed under the standard dictator game.

We find that agents redistribute higher amounts than principals under endogenous delegation which goes against the results observed under compulsory delegation. One possible explanation for this result concerns the agents beliefs on the principals behaviour. Previous literature states that the knowledge of having an extra competitor (the principal), may affect agents behaviour and result in lower competition (Garcia and Tor, 2009). We mitigate this effect by running two further endogenous delegation treatments: one where agents know that the principal can delegate to them or make the decision themselves and another (informationally closer to compulsory delegation) in which agents do not know that there is an alternative to choosing an agent. That is, they are *only* informed that the principal chooses to delegate or not. We find no differences between these two endogenous delegation treatments. We can thus conclude that agents behaviour could not be only explained by differences in information.

Another way to reconcile our results is by arguing that social distance (Hoffman et al. 1996) under endogenous and compulsory delegation is different. That is, under delegation the social distance is greater than under no-delegation. This implies that compulsory delegation favours selfish behaviour whereas, endogenous delegation will favour more ‘pro-social’ behaviour. Similarly, using the ‘responsibility hypothesis’ (Charness, 2000), one can say that the principals may feel more responsible over the outcome in endogenous delegation where they can make the decision themselves effectively avoiding an unfair distribution. However, both responsibility and social distance cannot fully reconcile our results either. We are unable to explain why the allocations are lower when principals choose to make the allocation decision on their own.

Literature Review

Previous literature has used the well-known dictator game (Forsythe et al., 1994) to study the distributional consequences of delegation (Hamman et al. 2010; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013). The main result from this literature is that compulsory delegation results in less equalitarian outcomes than the standard dictator game. Others have studied delegation using the ultimatum game bargaining situation (Fershtman and Gneezy, 2001) and labor markets environment (Fehr et al. 2010; Charness et al. 2012 and Maximiano et al. 2013). Bargaining literature finds that delegation is a way of increasing the proposers share only if delegation is forced. Meanwhile in labour markets delegation can be used to make workers feel more attached to the company as workers on which principals delegate, are more likely to provide higher effort levels.

Dictator games have been extensively studied in experiments. The main results from a meta-study of more than 120 studies (see Engel, 2010) is that the average amounts redistributed to recipients represent the 28% of the total pie.¹ Some studies have suggested that these amounts may increase if social

¹Similar average amounts are obtained by Camerer (2003) who analysed the data of 11 studies and by Cardenas and Carpenter (2008) who perform a similar analysis with dictator games conducted in developing countries.

distance between principals and recipients decreases by identifying principals by their surname (Charness and Gneezy, 2008) or making principals feel that anonymity is not ensured (Hoffman et al. 1996). Furthermore, framing effects also vary the amounts transferred to recipients. Branas-Garza (2007) finds that emphasizing the dictators' responsibility over the outcome increases dictators' generosity by adding to the dictator's instructions the sentence "Note that he relies in you". Meanwhile, List (2007) finds that framing affects the outcome of a dictator game. In one treatment, principals were asked to split the endowment while on the other, they were asked to "take" money from recipients. This change in framing affected the outcome as principals transferred less money to recipients when they were in the "take" treatment.

Hamman et al. (2010) introduce a variant of the dictator game where principals delegate to agents and find that agents who share less with recipients are selected more often. This is in line with their claim that delegation is a tool used by principals to hide behind agents when unfair decisions have to be taken. The dictator game has also been used to study the effect of agents or principals being punished by their actions. Bartling and Fischbacher (2012) use groups of one dictator, one agent and two recipients and give one of the recipients the power to punish other players for their actions. They obtain that punishers use their power on both dictators and agents only when the outcome is unfair. Oexl and Grossman (2013) further extend Bartling and Fischbacher (2012) and find that delegation is effective in shifting the blame from principals to agents. Coffman (2011) also finds that intermediation reduces principal's punishment when the punishers payoff is independent of the principals actions, delegation is a useful mechanism when one wants to shift the blame for taking unfair decisions. On one side, the principal shifts the blame by making the agent decide, while the agent feels that it is the principal that pushes towards the unfair outcome. Garofalo and Rott (2017), also find that recipients punish both principal and agent for unfair decisions when the agents' only role is to communicate the decision made by the principal. Fershtman and Gneezy (2001) use an ultimatum game in which proposers can delegate the offer made to the recipient to an external agent. They find that when delegation is chosen the payoffs of the proposers increase. Choy et al. (2015) also use the ultimatum game to compare exogenous and endogenous delegation in a bargaining environment and find that unfair outcomes are more likely under endogenous delegation than

under compulsory delegation. They also find a difference between endogenous and exogenous delegation. Overall there is a common theme in all these papers, *delegation increases payoffs of the proposer/sender under delegation*.

There are other papers that have studied the joint effect of delegation and other factors such as dishonesty, corruption, information, gender² or bargaining. Erat (2013) and Sutan and Vranceanu (2015) examined dishonesty in environments with endogenous delegation of decisions. Sutan and Vranceanu (2015) use a dictator game in which proposers can lie about delegating to a third party. They find that imperfect information³ increases the proposers profit by shifting its blame.⁴ Similarly, Erat (2013) showed that agents are more frequently hired when they have to lie in a sender-receiver game.⁵

The rest of the paper is organized as follows. Section 3 describes our experimental design in detail. Section 4 presents our main results. Section 5 concludes.

²Bottino et al. (2016) show that in a compulsory delegation treatment female and male principals behave similarly while as agents' females show greater redistributive concerns relative to their male counterparts even though it is detrimental to them as they are selected less often.

³Lai and Lim (2012) study the effect of information and communication on delegation (without cheating option) and find that generally principals under-delegate even when is more profitable to do so. Furthermore, Cettolin and Riedl (2010), use delegation to prove that under uncertainty there is a violation of rationality in decisions.

⁴On this line, Jacquemet (2005) and Drugov et al. (2014) examine how intermediaries increase corruption by diminishing the moral cost of being corrupt by effectively shifting the blame of being corrupt.

⁵Surprisingly dishonesty on the sender-receiver game with agent is prevalent even when the identities of cheaters are revealed to other players (Van de Ven and Villeval, 2015).

Experimental Design

A total of 236 subjects were recruited via ORSEE (Greiner, 2015) to participate in the experiment at Middlesex University London⁶. Each subject participated in only one treatment. The experiment lasted for approximately 45 minutes and subjects earned on average £12. We conducted a total of 26 sessions (see Table 4.1 for a summary of the experimental design).

The experiment consisted of four treatments and three main conditions. Upon arrival participants were randomly allocated seats, read the instructions and were informed of their roles in the experiment. In the baseline treatment 10 or 8 subjects played a dictator game for 12 rounds.⁷ Half of the players were recipients and half principals. The delegation treatments included also two subjects in the role of agent. Therefore there were three possible roles: principal (player P), agent (player A) and recipient (player R).⁸

We have a total of four treatments and three different delegation mechanisms (See Figure 4.1). Treatments:

- **Baseline (BS)**

The BS treatment is the standard dictator game. In each session half of the subjects are assigned the role of principal and the other half the role of recipient. In each round, principals are told that they have to divide an endowment of £10 between themselves and a randomly matched recipient. Principals are informed that they will be matched with different recipients in each round. The treatment had 40 participants playing in 4 sessions.

⁶Instructions can be found in Appendix D.

⁷The group size depended upon show up.

⁸To avoid framing, in the instructions we referred to the participants as “A”, “B” and “C” instead of “principal”, “recipient” and “agent” respectively.

- **Endogenous Delegation (ED-1)**⁹.

In this treatment we introduce a third player, the agent (A). Principals can choose one of the two available agents (A1 or A2) or to make the decision of dividing £10 themselves. If the principal selects one of the two agents, this agent will then divide the endowment between principal and recipient. This treatment had 96 participants and 11 sessions.

In this treatment we explicitly inform the agents that principals can delegate or make the decision themselves. Agents thus know that they are competing among themselves and the principal. Compared to compulsory delegation agents have an extra competitor, the principal. The knowledge of this extra competitor, at the same time the decision maker, may generate beliefs (of agents) on the principals affecting agents behavior and resulting in diminished competition (Garcia and Tor, 2009). We thus ran a third treatment where we made the ED treatment informationally closer to the CD experiments. That is, we only inform the agents that the “*principal makes the decision to delegate to either one of them*”. Informationally, this treatment lies between CD and ED-1. This is the treatment below: ED-2.

- **Endogenous Delegation without Information (ED-2)**

In ED-1 agents knew that compared to compulsory delegation, they had an extra competitor, the principal. In ED-2 agents do not know that principals can divide the £10 themselves (see instructions in Appendix D). This new treatment is created in order to see if the knowledge of having an extra competitor, affects agents behaviour. We had 56 participants playing in 6 different sessions.

⁹Following Hamman et al (2010) we also performed a treatment in which subjects participate in a CD treatment for 6 rounds and ED-1 during the last 6 rounds. We replicated this treatment for consistency reasons. Our results replicate those of Hamman et al. (2010) where they observe that the behaviour in CD and ED rounds is similar. In both cases the amounts given to the recipient are low and decrease over time. The results of this treatment are in Appendix C.

- **Compulsory Delegation (CD)**

In this treatment we replicate Hamman et al. (2010). Delegation is compulsory and therefore the principal has to select an agent to divide the endowment. Each session has two agents: A1 and A2. The principals select one of the two agents to divide the endowment in each round.¹⁰ We run 5 sessions with a total 44 participants.

Our payment scheme for all the treatments is a variation of the one in Hamman et al. (2010).¹¹ Both principal and recipient were paid in cash for one randomly selected round drawn at the end of the experiment. Agents were paid differently. In addition to the £5 show-up fee they obtain an additional £5 at the beginning of the experiment, starting capital. Their experimental payment was calculated as follows:

$$\pi_i = -0.30 + 0.15n_i$$

Where, 0.30 represents the fixed costs that agents face in each period regardless of them being selected by any principal. n_i is the number of principals choosing agent i . In addition to the experimental earnings, all subjects were paid a £5 show-up fee plus £2 for completing a series of questionnaires after the experiment. At the end of the experiment each participant received a sealed envelope with their identification number and the amount they earned.

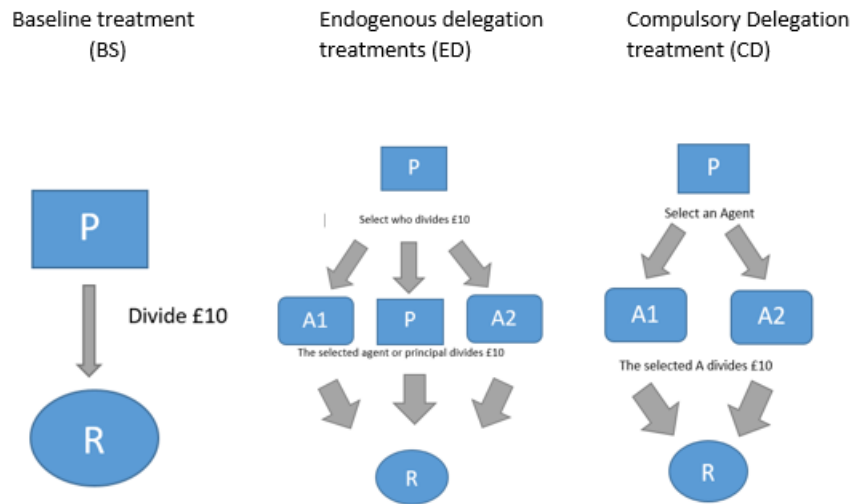
¹⁰In Hamman et al. (2010), principals were randomly allocated to an agent at the beginning of period 1, introducing the possibility of choosing one agent or the other from round 2 on. This study will differ as principals choose an agent on every possible round, including round 1. We chose not to impose one round of compulsory delegation before making delegation optional in order to avoid any effect from the initial round.

¹¹We modified the coefficients of the payment equation to adapt it to the amount of players that we had per session.

Table 4.1: Summary of the experimental design

	Baseline (BS)	Endogenous Delegation (ED-1)	Endogenous Delegation (ED-2)	Compulsory Delegation (CD)
Divides the endowment	Principal —	Principal Agent	Principal Agent	— Agent
Sessions	4	11	6	5
Number of subjects	40	96	56	44
Principals/Agents /Recipients	20/-/20	37/22/37	22/12/22	17/10/17

Figure 4.1: Summary of the experimental design.



Previous literature finds that allocations under delegation favour the principal (Hamman et al. 2010; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013) and that principals select agents that maximize their pay-offs (Hamman et al. 2010; Bottino et al. 2016). Based on this our main hypotheses are:

- **Hypothesis 1:** *Amounts allocated to recipients will be lower under delegation than under no-delegation.*
- **Hypothesis 2:** *Under endogenous delegation allocation to recipients will be lower when principals delegate compared to when they make the allocation decision themselves.*
- **Hypothesis 3:** *Agents making allocation decisions favourable to the principal will be selected more often under delegation.*

Results

Overall we find that ED-1 and ED-2 treatments give us the same results (see Appendix A for a more accurate comparison between these two treatments) regarding the amount redistributed to recipients, the proportion of decisions delegated and the behaviour of agents. We decided to pool the results of these two treatments. From now on we will refer to this data simply as ED. Further, we refer to those who delegate under ED as ED-D and those who do not as ED-N.

Hypothesis 1 predicts that amounts allocated to recipients will be lower under delegation. Figure 4.2 shows the amount transferred to recipients by round in each of the treatments: BS, CD and the two choices of delegation that the principals have in ED: ED-D and ED-N. We see that BS follows a very similar pattern as ED-D. A Mann-Whitney (rank-sum) test of distributions¹² confirms that the populations of BS and ED-D follow the same distribution ($z=0.000$, $p=0.99$). Meanwhile ED-N does not follow the same distribution as BS ($z=2.811$, $p=0.00$) and ED-D ($z=-3.252$, $p=0.00$). When

¹²From now on we use Mann-Whitney non-parametric tests to study the hypothesis that two independent samples are from populations with the same distribution.

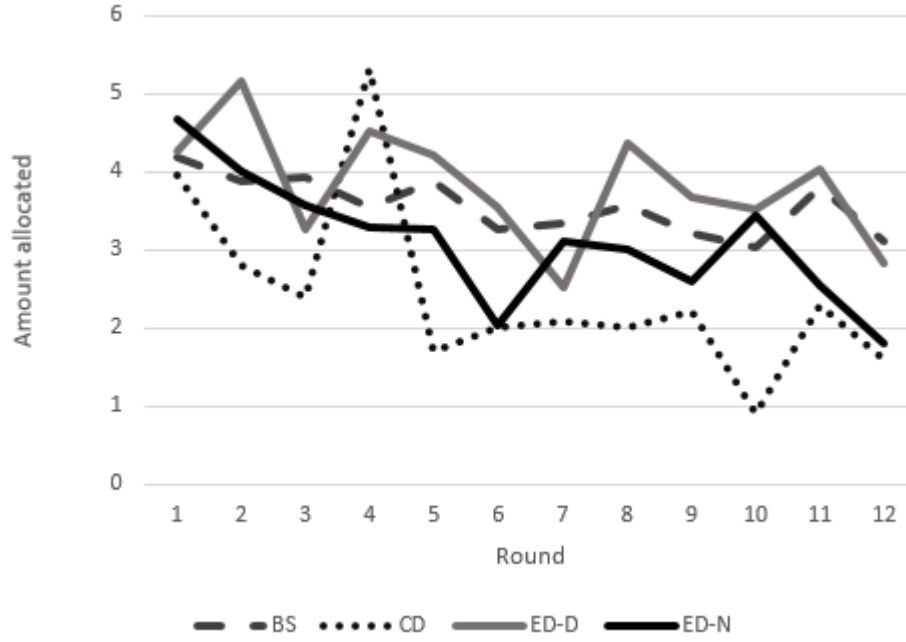
principals make the allocation decision in ED-N, they allocate lower amounts to recipients. Still, the amounts allocated in ED-N are significantly higher than the amounts allocated in compulsory delegation ($z=-2.550$, $p=0.01$).¹³
¹⁴ This means that the amounts redistributed in CD are lower than in the other treatments. The results of ED-D therefore contradict Hypothesis 1.¹⁵
This gives us Result 1.

¹³The amounts allocated to recipients are also higher in BS than in CD ($z=6.029$, $p=0.00$).

¹⁴A quantile regression analysis with the amount allocated as a dependent variable and all the treatment dummies (but CD) as independent variables gives us that all the coefficients are significant and with positive values BS: 2.18, ED-D: 1.23, ED-N:1.18. Therefore, higher amounts are allocated in all the treatments compared to CD.

¹⁵Notice that we can check for how the distribution changes across the periods and treatments using the Gini index, a measure of inequality. We obtain that the Gini coefficient has a higher value in CD (0.44) than in BS (0.29), ED-N (0.30) or ED-D (0.16) where the Gini coefficient is the smallest. This shows that, CD generates more inequality than the other two treatments. This is interesting as it points out that removal from decision making is not always detrimental to income equality.

Figure 4.2: Amounts earned by the recipient by round and treatment



Result 1

- **a:** *Amount allocated to recipients is higher when principals choose to delegate than when they make the allocation decision themselves.*
- **b:** *Allocations made by agents under endogenous delegation converge to the one made by principals in the standard dictator game.*
- **c:** *Under compulsory delegation recipients earn significantly less than in the baseline and either endogenous delegation treatments.*

Hypothesis 2 states lower redistribution when agents are selected under endogenous delegation. In Table 4.2 we find that delegation occurs in roughly the 50% of the cases.¹⁶ Surprisingly, the average amount allocated

¹⁶Figure 4.1 in Appendix B, shows a histogram dividing the principals by number of

to the recipient by principals (3.14) is significantly lower than the allocation made by agents (3.83).¹⁷ Given the results from previous literature this is unexpected. We find that agents allocate more to recipients than principals when we expected the opposite. Compared to CD, where agents compete by giving lower amounts to recipients, the situation seems to change when ED is implemented. This contradicts Hypothesis 2 and gives us Result 2.¹⁸¹⁹

- **Result 2:** *Delegating does not result in more unfair distributions under endogenous delegation. In fact, agents reallocate higher amounts to recipients than principals.*

Table 4.2: Percentage of delegated decisions and corresponding amount re-distributed per group of rounds.

Rounds	Proportion of delegated decisions	Average amount allocated	
		Agent	Principal
(1-4)	49%	4.31	3.91
(5-8)	52%	3.66	2.97
(9-12)	53%	3.5	2.60

times that they decided not to delegate. We observe that the distribution is very close to a normal distribution and that subjects choose to delegate in roughly half of the cases (5.98 is the mean amount of times that one subject does not delegate).

¹⁷A Mann-Whitney test confirms that the amounts transferred to the recipient in ED-D and ED-N treatments are statistically different over time ($z=3.252$ $p=0.00$).

¹⁸Figure 4.2 in Appendix B, shows that there is not a clear tendency regarding the average amount allocated to the recipient by number of times that a person decides not to delegate in the ED treatment. A t-test, however confirms that the average amount transferred by people who delegate in more than half of the rounds is statistically different from the quantity transferred by those who choose to delegate in less than half of the rounds ($t= 2.914$, $p=0.00$). Principals who delegate more earn more.

¹⁹We ruled out potential informational asymmetries affecting our results by running ED-1 and ED-2 and obtaining the same results.

Now, we focus our attention on Hypothesis 3. It predicts that agents making decisions favourable to the principal are selected more often. To check this we investigate agent behaviour in the delegation treatments. We find that the average shared amount by agents that are not chosen by principals in the next round in CD is 3.88. This is well above the average amount shared by those who are selected again, 1.81. Under ED principals seem to react less to lower amounts. Principal's switch the decision maker when the amount allocated is on average 3.47 and they do not change their delegation strategy when the amount is 2.86 (the difference is lower but still significant $t=-2.320$, $p=0.02$). Moreover, principals switch the decision maker more often in ED (52%) than in CD (36%).²⁰ Notice that in ED they also have more alternatives when they decide to switch the decision maker.

To better understand the mechanism by which agents are chosen by principals we performed a series of logistic regressions (clustered by subject) for CD and ED treatments (Table 4.3).²¹ Model 1 and Model 2 analyse the CD data, their dependent variable is a dummy with value one if the principal decides to switch agents after that round and zero otherwise. In Models 3, 4, 5, 6 the data examined is from ED. Model 3 and 4 have as dependent variable a dummy with value one if principals switch from delegating to not delegating and zero otherwise. The dependent variable in Model 5 and 6 is a dummy with value one if the agent changes the decision maker in any way (from agents to principal, principal to agents or from one agent to another) from one round to another, and zero otherwise. The explanatory variables are the same for all the models: the amount allocated to recipients on the previous period and the rounds (only on models 2, 4 and 6).

²⁰Figures 3 and 4 in Appendix B present the proportion of switching in decision maker under ED and CD over time. Switching decreases over time.

²¹Since the dependent variables are always categorical we use logistic regressions. We also excluded the last round from the regressions to avoid the end game effect.

Table 4.3: Switching in round t based in round t-1

	CD		ED			
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Amount allocated to recipients on the previous round	0.28*** (0.08)	0.29*** (0.08)	0.08*** (0.03)	0.08*** (0.02)	0.13*** (0.00)	0.13*** (0.00)
Round		0.05 (0.04)		-0.01 (0.23)		-0.02 (0.23)
Constant	-1.29*** (0.32)	-1.63*** (0.50)	-1.13*** (0.19)	-1.08*** (0.26)	-0.32*** (0.21)	-0.15*** (0.24)
Observations	187 (17)	187 (17)	649 (59)	649 (59)	649 (59)	649 (59)
Log likelihood	-99.46	-110.57	-395.88	-395.84	434.28	-433.65
Prob > χ^2	0.00	0.00	0.01	0.02	0.00	0.00

, where the number in parenthesis represent the standard errors for each coefficient. *** p<0.01, ** p<0.05, * p<0.1.

In Models 1 and 2 the variable amount allocated to recipients in the previous round is positive and significant (0.28 and 0.29 respectively) at a 1% level. This indicates that in CD principals switch agents in a round when the previously selected agent allocated higher amounts to the recipient. Agents that are selected are those allocating lower amounts to recipients.

Models 3 and 4 provide us a lower coefficient associated with the amount given in previous periods by agents (0.08), this coefficient is also significant at a 1% level. Principals in this case, switch from delegating to not delegating when the amount allocated in the previous round is low. Meanwhile, Models 5 and 6 indicate that in ED higher amounts allocated to the recipient.

ent mean a higher likelihood of a change in strategy in the next round. Note that the size of the coefficient associated with the amount transferred (0.13) is smaller than in the CD treatment. In ED principals still switch due to higher amounts redistributed, but the effect is considerably smaller than in CD. The coefficient associated with the round is never significant.

This result is in line with the previous literature (Hamman et al. 2010; Bartling and Fischbacher, 2012) where they argue that principals select those agents who share less with recipients. Regardless, the coefficients associated with the amount shared are smaller for ED regressions than for CD. Under CD principals are more prone to switch agents if the amount earned is small, this hints that competition between agents in CD is greater than under ED. It is not clear why this occurs. One potential explanation is that agents in CD understand that principals switch agents when they transfer more to recipients and thus start transferring lower amounts each round. Under ED this is not the case. Principals also switch their strategy when they get lower amounts, but in a more moderate way. Agents do not seem to realize about the relationship between lower amounts and switching strategy in ED and therefore do not use allocating lower amounts to recipients as a strategy to be selected by agents over time.²² This gives us Result 3:

- **Result 3:** *In endogenous delegation, principals switch agents when they allocate higher amounts to recipients, however, this reaction is small and as a result agents do not reallocate lower amounts to recipients. Under compulsory delegation principals select those agents transferring less to recipients and the effect is bigger than in endogenous delegation.*

Conclusions

The main results from this paper is that endogenous delegation does not increase inequality between principals and recipients. A priori we expected that when agents made the decision (delegation), the allocation to recipients

²²When we analyse the data from agents perspective we also find a clear relationship between higher profits for agents and lower amounts reallocated to recipients in CD. We do not find this in ED. See Table 4.2 in Appendix B for a detailed analysis of agents behaviour.

would be lower than when principals decided (no delegation). Surprisingly, agents in endogenous delegation are the ones allocating higher amounts to recipients. This contradicts previous literature and our results under compulsory delegation that find that principal's act as profit maximizers and tend to select agents who share lower amounts with recipients.

Clearly there is a behavioural difference of agents behaviour both in compulsory and endogenous delegation treatments. This could be explained by the endogenous mechanism or by the different information structures of both games. In compulsory delegation agents were competing among themselves while in endogenous delegation the principal could choose not to delegate. This may generate beliefs (of agents) on the principals. A priori this suggests that competition for agents is greater under endogenous delegation. We ruled out any possible information effect that could explain this results implementing two different endogenous delegation treatments, one with information on principals possible choices and another on which agents did not know that they were also competing with the principal (informationally closer to compulsory delegation). The results of these two treatments are identical suggesting that information asymmetries were not affecting the choices of principals and agents.

Previous literature has mainly focused on compulsory delegation (Haman et al. 2010) finding that principal's act as profit maximizers under this structure opposed to their behaviour on a simple dictator game where they tend to show a more altruistic behaviour with recipients. Both baseline (dictator game) and compulsory delegation treatments' results are consistent with this existing literature.

As earlier mentioned, one can partly reconcile our results with earlier outcomes by appealing to the social distance (Hoffman et al. 1996). Under compulsory delegation an agent is imposed, increasing the social distance with respect to the standard dictator game. This results in prosocial motives being removed and lower quantities being reallocated to recipients. However in endogenous delegation social distance is weaker as the principal can choose not to delegate. This results in higher redistribution. Along a similar line, another explanation could be the role of responsibility (Charness, 2000). The responsibility of the decision lies in the principal more under en-

ogenous delegation as the principal can actively avoid unfair situations. In addition, previous studies (Dana et al. 2006; Dana et al. 2007; Andreoni and Bernheim, 2009; Grossman 2014) find that subjects do not always want to face the outcome of an unfair decision. In compulsory delegation principals do not directly choose the unfair outcome and therefore they do not directly have to face the earnings of the recipient (even if they can easily calculate them) and this affects them feeling less responsible over it.

Both, social distance and responsibility hypothesis, would imply lower allocations to the receivers when one delegates. However, under endogenous delegation, allocations are lower when the principal chooses not to delegate where implied social distance is lower. The explanation then has to lie in the fact that in our scenario delegation may not be a test for passing the responsibility for bad decisions. We are further running experiments under different frameworks where we address this directly.

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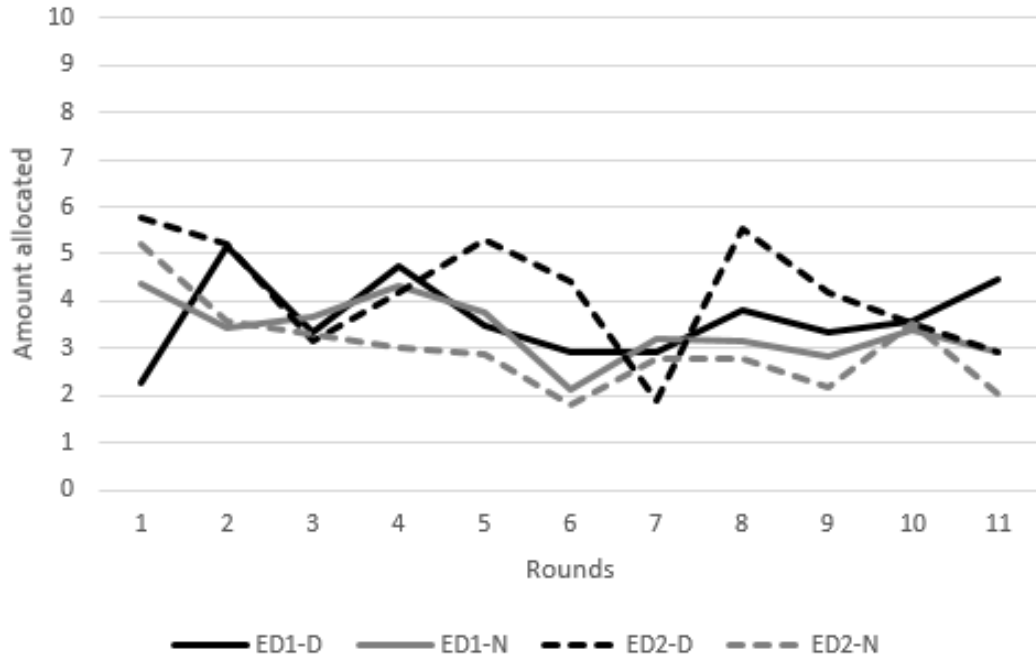
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Appendix A

Figure 4.1: Amounts earned by the recipient by round when they delegate and not in ED-1 and ED-2 treatments



Overall we find that ED-1 and ED-2 give us the same results. The average shared amount with recipients in ED-1 is 3.47, not statistically significantly different from the average transferred amount in ED-2 , 3.49 ($z=0.731$, $p=0.464$ in a Mann-Whitney test).

In both treatments roughly half of the decisions were delegated, hinting that principals' behavior did not change either. Moreover, as we can see in Figure 4.1, in both treatments selected agents reallocated higher amounts to recipients than principals, ED-1: 3.28 if they do not delegate, 3.70 when they delegate ($z=1.273$, $p=0.20$); ED-2: 2.89 when they do not delegate, 4 when

they delegate ($z=3.866$, $p=0.01$). We thus conclude that the information in the experiment does not matter in terms of outcomes. Given this we pool the data from these two treatments. Henceforth this pooled data is referred to as ED.

Appendix B

Figure 4.1: Number of times that each principal chooses not to delegate.

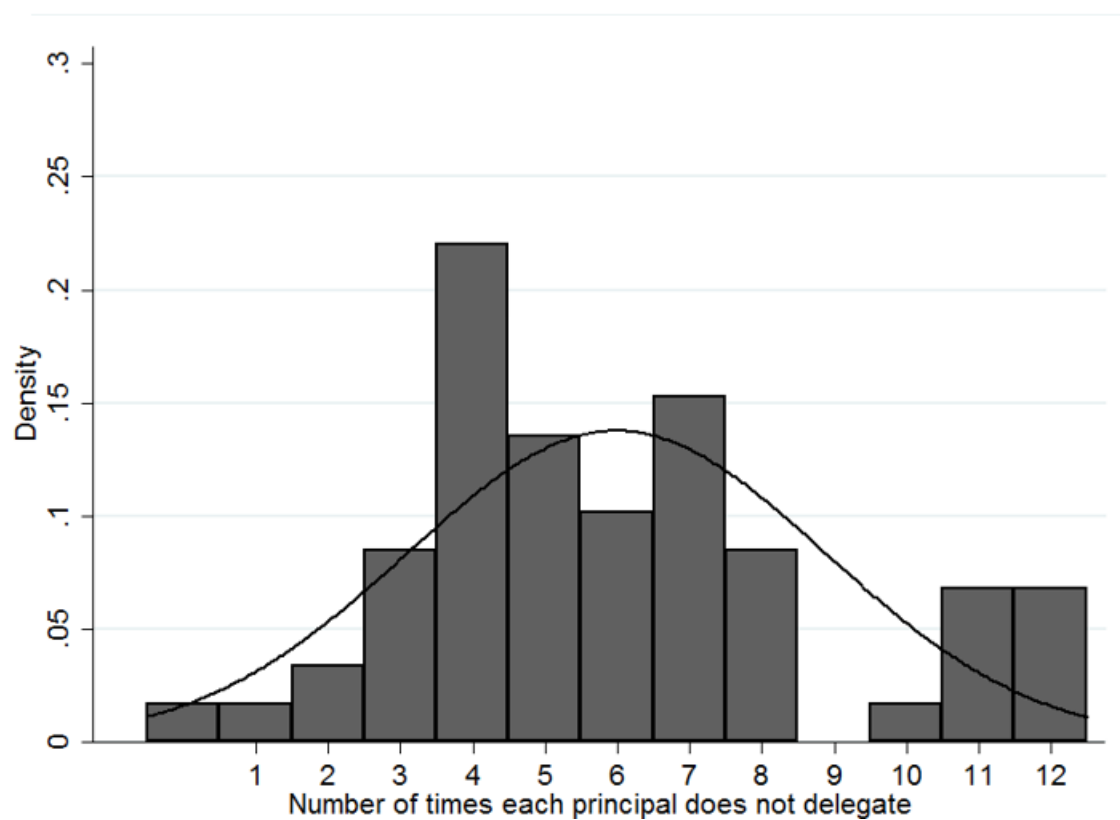


Figure 4.2: Average amounts transferred to the recipient by number of times that the subject decided not to delegate.

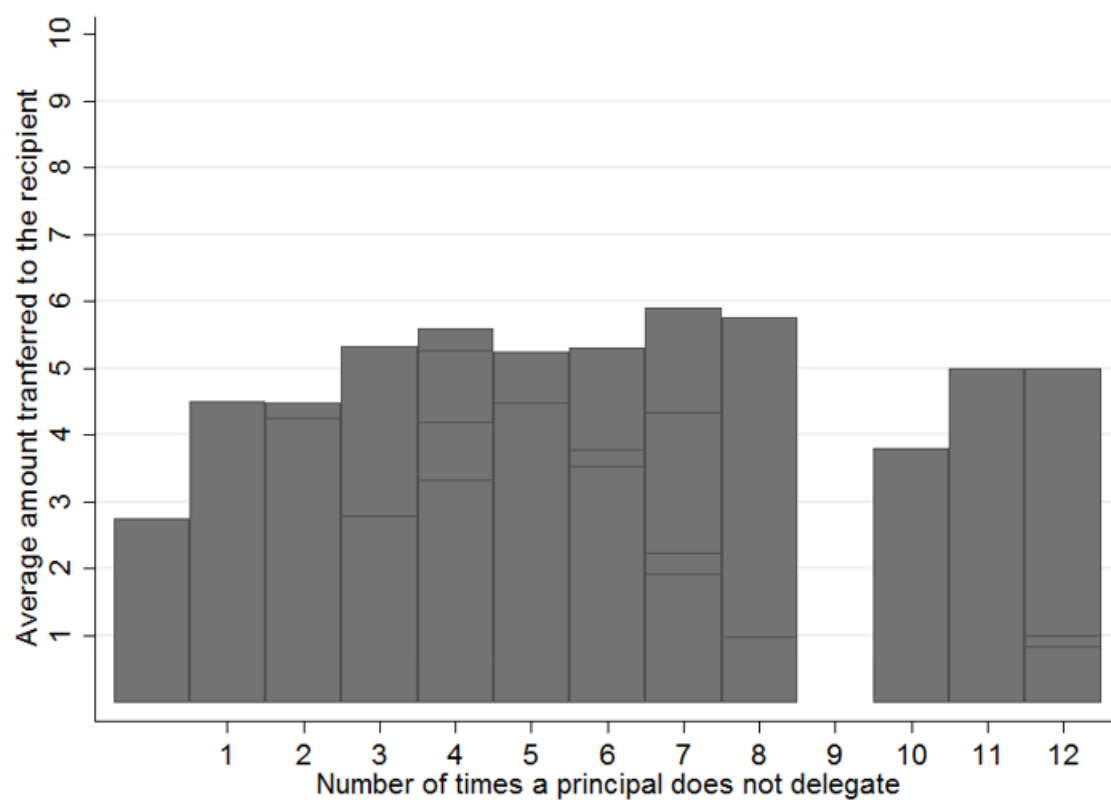


Figure 4.3: Percentage switching from one decision maker to another by round in CD.

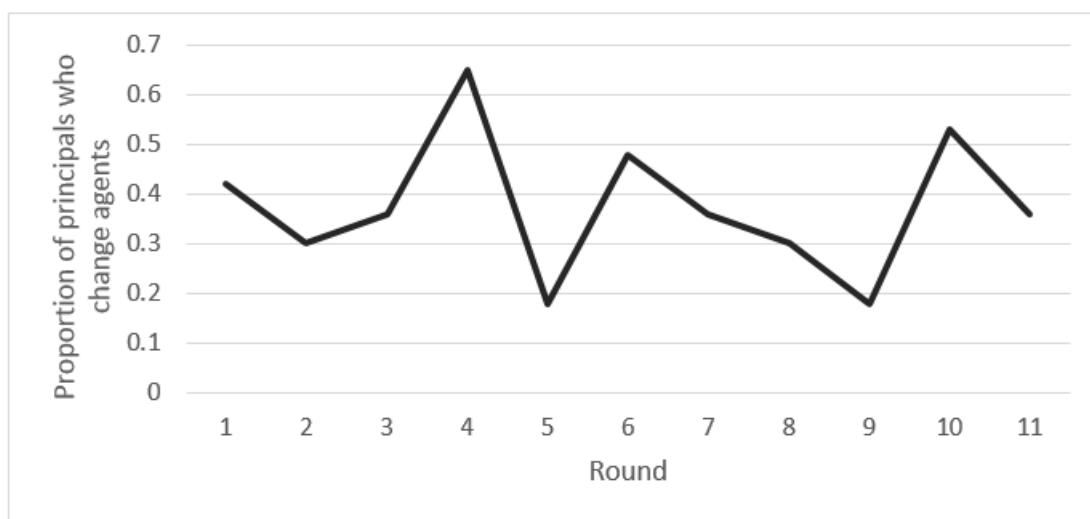


Figure 4.4: Percentage switching from one decision maker to another by round in ED.

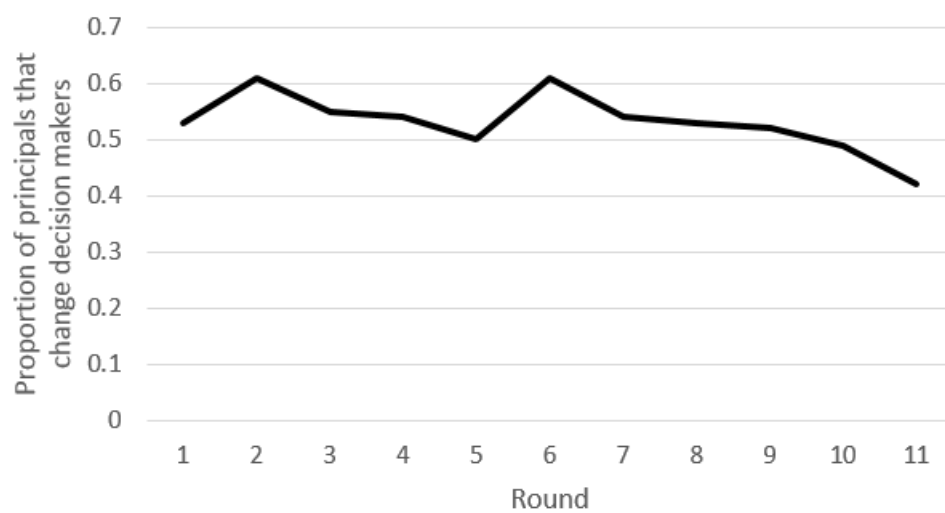


Table 4.1: Answers to the questionnaire

	BS		CD			ED		
Player	P	R	P	R	A	P	R	A
I feel involved in this experiment	1.85 (0.12)	-0.78 (0.14)	0.17 (0.15)	-0.82 (0.13)	0.88 (0.21)	1.30 (0.06)	-0.43 (0.10)	-0.27 (0.11)
Total profit obtained was relevant for me	1.11 (0.07)	0.68 (0.10)	1.57 (0.09)	0.05 (0.10)	0 (0.33)	1.22 (0.05)	0.91 (0.09)	0.03 (0.09)
I consider my behaviour acceptable	2.25 (0.07)	2.68 (0.08)	2.64 (0.05)	1.82 (0.08)	2.87 (0.03)	1.66 (0.06)	2.32 (0.06)	1.64 (0.08)
Players P with which I interacted had an acceptable behaviour	—	0.10 (0.12)	—	-0.23 (0.09)	0.70 (0.08)	—	0.56 (0.09)	0.44 (0.09)
Players A with which I interacted had an acceptable behaviour	—	—	1.86 (0.07)	—	—	1.05 (0.05)	—	—
I am ready to take risks	1.46 (0.09)	1.73 (0.8)	1.94 (0.07)	1.76 (0.05)	2.30 (0.10)	1.35 (0.05)	1.89 (0.06)	1.87 (0.07)

Note: Average values of the answers to a seven item scale questionnaire, from strongly disagree (-3) to strongly agree (+3) with a neutral option (0). Standard errors in parenthesis.

Table 4.2 presents two regressions using the data from agents. The dependent variable will be the average allocation made by agents a round while the set of independent variables will include if the agent sees that he has been less chosen and therefore earned less profit (profit) and the round they are in. Generalized least squares regressions with cluster-robust standard errors are used as estimation method. In Model 1, the data analysed is CD and in Model 2 ED.

There is a clear relationship between higher profits for agents and lower amounts reallocated to recipients in CD as the coefficient associated to agents profit is negative and significant. We do not find this in ED as the coefficient associated to profits is not significant.

Table 4.2: Amounts reallocated by agents depending on the profit obtained by them per round

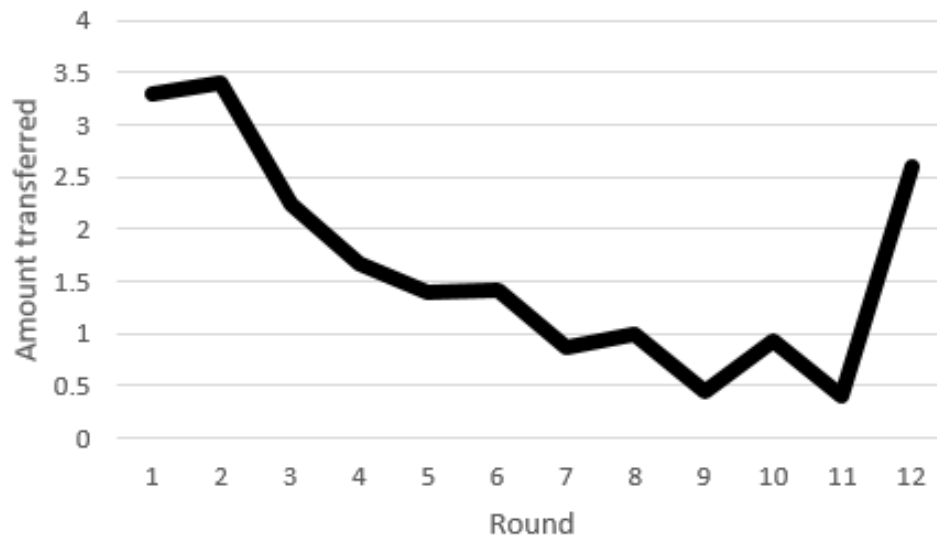
	CD	ED
	Model 1	Model 2
Profit	-3.97* (0.05)	-0.16 (0.48)
Round	-0.06 (0.39)	-0.09** (0.04)
Constant	3.09*** (0.00)	4.25*** (0.31)
Observed agents	10	34
Prob > χ^2	0.01	0.23

,where the number in parenthesis represent the standard errors for each coefficient. *** p<0.01, ** p<0.05, * p<0.1

Appendix C:

Compulsory and Endogenous delegation extra treatment. This graph presents the results of a 6 round CD and 6 rounds ED treatment. The treatment had 70 participants playing in 5 different sessions. We obtain the same results as in Hamman et al. (2010), where the outcome of ED after some rounds of CD converge to CD.

Figure 4.1: Average amounts transferred to the recipient by round in CD-ED extra treatment



Notice that subjects played CD from round 1-6 and ED from round 7-12.

Appendix D: Instructions

Treatment 1: Baseline (BS)

Thanks for participating in the experiment!

Please remember that from this moment on and till the end of the experiment no communication is permitted. If you have a question at any moment please raise your hand and we will answer your question in private.

All identities in this experiment are anonymous. No one in the experiment will get to know your identity during or after the experiment. Your identity will only be used to ensure that you are paid correctly. Please read the instructions with care. After reading them and before starting the game we will provide each participant with a graphical example of the experiment.

You will be paid at the end of the experiment. During the experiment we will use the term experimental money (EM) to refer to your earnings. At the end of the experiment we will transform this amount in the local currency using an exchange rate of 1. Notice that we will add 5 EM show up fee to your experimental earnings. Your earnings are your private information.

Experiment:

The experiment has twelve periods. The structure of the experiment is the following.

There are two types of players in this experiment: player A and player B.

At the start of the experiment you will be assigned a personal identification number by the computer. Whether you will be Player A or B is determined (randomly) by the computer.

In each period, each player A is randomly paired with a player B. Each pair, A-B, will be assigned 10 EM. We will now explain the structure of the game.

Each period:

At the start of each period, player A decides how to allocate 10 EM to player B and herself. The allocations can be made in increments of 1 cent of EM. The amount of 10 EM will be fully allocated between players A and B. That is, the amount assigned to player B plus the amount player A decides to keep will always add to 10 EM.

Once player A has taken their decision, each player will be informed about the amount they have been assigned.

At the end of each period all players, A and B, will see the information regarding current and previous periods, the identification numbers of their pairings and the amount assigned to them in each period.

Payment:

Besides the 5 EM show up fees, each participant will be paid in the following manner.

At the end of the experiment one of the periods will be randomly chosen for each player A and B. Each player will be paid the amount they earned in that period. In addition, subjects will earn 2 EM for completing two short questionnaires

You will be called individually at the end of the experiment to be paid. You will inform the experimenter about your ID number and will be paid accordingly.

Any questions?

Treatment 2: Endogenous Delegation (ED-1)

Thanks for participating in the experiment!

Please remember that from this moment on and till the end of the experiment no communication is permitted. If you have a question at any moment please raise your hand and we will answer your question in private.

All identities in this experiment are anonymous. No one in the experiment will get to know your identity during or after the experiment. Your identity will only be used to ensure that you are paid correctly. Please read the instructions with care. After reading them and before starting the game we will provide each participant with a graphical example of the experiment.

You will be paid at the end of the experiment. During the experiment we will use the term experimental money (EM) to refer to your earnings. At the end of the experiment we will transform this amount in your currency using an exchange rate of 1. Notice that we will add 5 EM show up fees to your experimental earnings. Your earnings are your private information.

Experiment:

The experiment has twelve periods. The structure of the experiment is the following.

There are three types of players in this experiment: player A, player B and player C.

At the start of the experiment you will be assigned a personal identification number by the computer. The computer will also inform you regarding the type of player you have been chosen to be.

In each period, each player A is randomly paired with a player B. There will also be two C players.

At the start of the period 10 EM will be assigned to each pairing (A-B). We will now explain the structure of the experiment.

Each period:

Player A's screen will show two boxes. Each box contains one of the two players C and the option Myself.

Each player A has to select if the decision of dividing the experimental money is taken by one of the two C players or by player A (option Myself). To select an option, Player A simply needs to click on the associated box.

If A chooses player C, then player C decides how to divide the 10 EM between each pair of players A and B. The allocations can be made in increments of 1 cent. The amount of 10 EM will be fully allocated between players A and B. That is, the amount assigned to player B and player A always adds to 10 EM.

The allocation made by player C to a pair (A,B) is independent of the allocation made to another pair (if player C has been chosen by more than one player). If a player C has not been chosen by any player (A) then their screen will show a waiting message.

If Player A chooses the option Myself, the division of the money will be done by A.

Once all players have made their decisions, each player A and B will then be informed about the amount they have been assigned. In addition, all players will see a table containing all the information regarding previous periods. Once all players A have made their decisions, player C is informed about the number of players A who have chosen him. Each decision maker (A or C) decides how to allocate 10 EM between players A and B. The allocations can be made in increments of 1 cent. The amount of 10 EM will be fully allocated between players A and B. That is, the amount assigned to player B and player A always add to 10 EM.

Payment:

Besides the 5 EM show up fees, each participant will be paid in the following manner.

At the end of the experiment one of the periods will be randomly chosen for each player A and B. Each player will be paid the amount they earned for that period. That is, this will be the amount that was allocated to them by player C in that period.

At the start of the experiment each player C is given an additional quantity of 5 EM. However, player C loses 0.30 EM in each period and earns 0.15 EM for each player that selects him. Player C's earnings are the total sum of the earnings in the entire experiment.

You will be individually called at the end of the experiment to be paid. You will inform the experimenter about your ID number and you will be paid accordingly.

Any questions?

Treatment 3: Endogenous Delegation without Information (ED-2)

Thanks for participating in the experiment!

Please remember that from this moment on and till the end of the experiment no communication is permitted. If you have a question at any moment please raise your hand and we will answer your question in private.

All identities in this experiment are anonymous. No one in the experiment will get to know your identity during or after the experiment. Your identity will only be used to ensure that you are paid correctly. Please read the instructions with care. After reading them and before starting the game we will provide each participant with a graphical example of the experiment.

You will be paid at the end of the experiment. During the experiment we will use the term experimental money (EM) to refer to your earnings. At the end of the experiment we will transform this amount in your currency using an exchange rate of 1. Notice that we will add 5 EM show up fees to your experimental earnings. Your earnings are your private information.

Experiment:

The experiment has twelve periods. The structure of the experiment is the following.

There are three types of players in this experiment: player A, player B and player C.

At the start of the experiment you will be assigned a personal identification number by the computer. The computer will also inform you regarding the type of player you have been chosen to be.

In each period, each player A is randomly paired with a player B. There will also be two C players.

At the start of the period 10 EM will be assigned to each pairing (A-B). We will now explain the structure of the experiment.

Each period:

Each player A has to decide to delegate or not the decision of dividing the endowment on one of the two C players. To select an option, Player A simply needs to click on the associated box.

If A chooses a player C, then player C decides how to divide the 10 EM between each pair of players A and B. The allocations can be made in increments of 1 cent. The amount of 10 EM will be fully allocated between players A and B. That is, the amount assigned to player B and player A always adds to 10 EM.

The allocation made by player C to a pair (A, B) is independent of the allocation made to another pair (if player C has been chosen by more than one player). If a player C has not been chosen by any player (A) then their screen will show a waiting message.

Once all players have made their decisions, each player A and B will then be informed about the amount they have been assigned. In addition, all players will see a table containing all the information regarding previous periods. Once all players A have made their decisions, player C is informed about the number of players A who have chosen him. Each decision maker (A or C) decides how to allocate 10 EM between players A and B. The allocations can be made in increments of 1 cent. The amount of 10 EM will be fully allocated between players A and B. That is, the amount assigned to player B and player A always add to 10 EM.

Payment:

Besides the 5 EM show up fees, each participant will be paid in the following manner.

At the end of the experiment one of the periods will be randomly chosen for each player A and B. Each player will be paid the amount they earned for that period. That is, this will be the amount that was allocated to them by player C in that period.

At the start of the experiment each player C is given an additional quantity of 5 EM. However, player C loses 0.30 EM in each period and earns 0.15 EM for each player that selects him. Player C's earnings are the total sum of the earnings in the entire experiment.

You will be individually called at the end of the experiment to be paid. You will inform the experimenter about your ID number and you will be paid accordingly.

Any questions?

Treatment 4: Compulsory delegation (CD)

Thanks for participating in the experiment!

Please remember that from this moment on and till the end of the experiment no communication is permitted. If you have a question at any moment please raise your hand and we will answer your question in private.

All identities in this experiment are anonymous. No one in the experiment will get to know your identity during or after the experiment. Your identity will only be used to ensure that you are paid correctly. Please read the instructions with care. After reading them and before starting the game we will provide each participant with a graphical example of the experiment.

You will be paid at the end of the experiment. During the experiment we will use the term experimental money (EM) to refer to your earnings. At the end of the experiment we will transform this amount in the local currency using an exchange rate of 1. Notice that we will add 5 EM show up fee to your experimental earnings. Your earnings are your private information.

Experiment:

The experiment has twelve periods. The structure of the experiment is the following.

There are three types of players in this experiment: player A, player B and player C.

At the start of the experiment you will be assigned a personal identification number by the computer. The computer will also inform you regarding the type of player you have been chosen to be.

In each period, each player A is randomly paired with a player B. There will also be two independent C players.

At the start of the period 10 EM will be assigned to each pairing (A-B). We will now explain the structure of the game.

Each period:

Player A's screen will show two boxes. Each box contains the ID's of one of the two players C. Player A needs to choose from one of the two players C by clicking on the associated box. The chosen C will decide how to allocate the 10 EM between players A and B.

The selected player C decides how to divide the 10 EM between each pair of players A and B. The allocations can be made in increments of 1 cent. The amount of 10 EM will be fully allocated between players A and B. That is, the amount assigned to player B and player A always adds to 10 EM.

The allocation made by player C to a pair (A,B) is independent of the allocation made to another pair (if player C has been chosen by more than one player). If a player C has not been chosen by any player (A) then their screen will show a waiting message.

Once players C have made their decisions, each player A and B will be informed about the amount they have been assigned. In addition, all players will see a table containing all the information regarding previous periods.

Payment:

Besides the 5 EM show up fees, each participant will be paid in the following manner.

At the end of the experiment one of the periods will be randomly chosen for each player A and B. Each player will be paid the amount they earned for that period. That is, this will be the amount that was allocated to them by player C in that period.

At the start of the experiment each player C is given an additional quantity of 5 EM. However, player C loses 0.30 EM in each period and earns 0.15 EM for each player that selects him. Player C's earnings are the total sum of the earnings in the entire experiment.

You will be individually called at the end of the experiment to be paid. You will inform the experimenter about your ID number and will be paid accordingly.

Any questions?

Chapter 5

Gender Biases in Endogenous and Compulsory Delegation

Abstract

We explore gender differences in compulsory and endogenous delegation using a modified dictator game. Under compulsory delegation principals of either genders earn significantly more than in a dictator game as they select those agents who share lower amounts with recipients. Male agents are the ones allocating less to recipients and therefore the ones selected more frequently. Under endogenous delegation principals earn similar amounts to principals playing a standard dictator game. Independent of their gender, half the principals decide to delegate. There are no significant differences between male and female agents on amounts allocated to recipients.

Introduction

Delegation is an important tool to shape future hierarchical structures of firms as successful agents will continue receiving responsibilities and will potentially be promoted and occupy top positions in the structure. Therefore gender biases in delegation and selection of agents may partially help to explain why females are under-represented in competitive environments such as labor markets where females reach top level positions less often than males (Gregory-Smith et al. 2014; De Paola et al. 2016) and earn less than males

with similar backgrounds and responsibilities (Booth et al., 2003; ; Kulich et al. 2011).

We study gender differences under compulsory and endogenous delegation using a modified dictator game (Hamman et al., 2010). We ask the following questions: do male and female principals behave similarly when they can delegate? Do different gender agents make the same allocation decisions when delegation is compulsory and optional? And finally, who is selected more often?

The only paper studying gender differences in delegation is Bottino et al. (2016). They use a dictator game that requires delegating to a male or female agent and find no gender differences in principals earnings. In addition, they find that male agents are selected more often as they reallocate lower amounts than their female counterparts who are more concerned by equality in distributions. This is specially true in later periods. In this chapter we study whether gender differences are maintained under endogenous delegation.

We study gender differences in compulsory and optional delegation using a modified dictator game (Hamman et al. 2010). The first treatment, baseline, is a standard dictator game. The second treatment, endogenous delegation allows the principal to delegate to the agent, one of which is a male and the other a female. The selected agent or principal divides the endowment. The last treatment is compulsory delegation. The principal has to select one of the two available agents (a male and a female). The selected agent will then decide how much to allocate to the recipient. The gender of agents and principals is never known by the rest of the subjects.

Overall we find that, under compulsory or endogenous delegation, there are no gender differences on principal's average earnings. Differences, however, arise between agents. Male agents are selected more often when delegation is compulsory as they share lower amounts with recipients (in line with Bottino et al. 2016). In addition, male agents are selected even more often by male than female principals. By contrast, under endogenous delegation the amount of male and female agents selected is more homogeneous. Females are selected roughly more often. Finally, switching from one round to

another happens more often when delegation is optional. Also male decision makers switch less often in both treatments.

Literature review

Behavioural gender differences have been observed by many previous studies. The literature review of Croson and Gneezy (2009) make clear that there are gender differences in risk (Eckel and Grossman, 2008), social (Eckel and Grossman, 1996a; Aguiar et al., 2009) and competition preferences (Gneezy et al., 2003; Nierdele and Vesterlund, 2007).

Previous literature in competition shows that females tend to underperform in competitive environments even when their abilities are similar to the ones of males. Gneezy et al. (2003) ask participants to solve some puzzles and observe that in the baseline treatment males and females solve similar amount of puzzles, while under a competitive payment scheme male performance improves while female performance remains constant.¹ The tendency of females to compete less is tested by Nierdele and Vesterlund (2007). They show that when possible 73% of males choose to compete while only 35% of females make the same choice.

Gender differences are also found in risk attitudes. Eckel and Grossman (2008) review the experimental literature on risk and gender finding that most studies find that females tend to be more risk averse than males.²

In dictator games (Forsythe et al. 1994) gender differences show mixed results. Bolton and Katok (1995) find no gender differences in dictator's behavior when the gender of the recipient is unknown. By contrast, Eckel and Grossman (1996a), conclude that all-female groups are more altruistic

¹Gneezy et al. (2009) perform a field experiment on which they test competition in a patriarchal and matrilineal society. They observe that females shy away from competition in a patriarchal society like ours. This differences in competitive attitudes do not take place in a matrilineal society.

²Booth and Nolen (2012) suggest that females are less risk averse when they are in all female environments.

than all-male groups when recipient's gender is salient. Andreoni and Vesterlund (2001) introduce a modified dictator game where prices and income are different. They show that, when altruism is expensive, women share higher amounts with recipients, but when it is cheap, men are more altruistic. Regarding subjects' expectations Aguiar et al. (2009) show that women expect women principals to be more generous while men do not predict any gender differences in generosity.

Bottino et al. (2016) also use a modified dictator game to explore gender biases in delegation and find that under compulsory delegation male and female principals behave similarly and appoint the agent who gives less to recipients. This results in both male and female principals delegating more to male agents over time.³

In this paper we extend Hamman et al. (2010) and Bottino et al. (2016) to investigate gender biases in endogenous delegation. The paper is organized as follows. The experimental design is explained in section 3. The main results are discussed in section 4. Section 5 concludes and discusses possible extensions.

Experimental design

The experiment run to obtain the presented data is the same used in chapter 4: "*Distributional consequences of Endogenous and Compulsory Delegation*". The detailed experimental design can be found in Chapter 4.⁴

The important aspect that should be added is that in all the sessions of CD and ED there were two agents, one male and one female. This information was not salient and therefore participants were unaware of this.

Table 5.1 summarizes the experimental design and shows the number of male (M) and female (F) principals and agents per treatment.

³For more details on the delegation literature go to Chapter 4.

⁴The instructions of this experiment can be found in the Appendix D of Chapter 4.

Table 5.1: Summary of the experimental design

	Baseline (BS)	Endogenous Delegation (ED)	Compulsory Delegation (CD)
Divides the endowment	Principal	Agent/Principal	Agent
Sessions	4	17	5
Number of subjects	40	152	44
Principals	20 (10M/10F)	59 (24M/35F)	17 (7M/10F)
Agents	-	34 (17M/17F)	10 (5M/5F)

From previous literature (Hamman et al. 2010; Bottino et al. 2016) and the specifications of our experimental design we will propose the following hypotheses:

- **Hypothesis 1:** *Principals of both genders will earn more in both delegation treatments compared to the standard dictator game. We will not find gender differences in principals earnings per treatment.*
- **Hypothesis 2:** *Male agents will be more successful in both delegation treatments since they will be the ones reallocating lower amounts to recipients.*

Results

Hypothesis 1 predicts higher amounts earned by principals in both CD and ED-D compared to BS and ED-N. We look at the average earnings of male and female principals per treatment (Table 5.2) and find that principals of both genders earn substantially more in CD than in any of the other treat-

ments.⁵ Moreover, the differences in earnings of male and female principals are only statistically different in the BS treatment where females earn more than males (Mann-Whitney test⁶ ; $z=-4.73$, $p=0.00$).⁷⁸

Table 5.2: Amounts earned by male and female principals by round

	Earnings male principals	Earnings female principals	Total earnings
BS	5.78 (0.19)	6.96 (0.18)	6.43 (0.14)
ED-D	6.35 (0.28)	6.01 (0.22)	6.17 (0.16)
ED-N	6.93 (0.25)	7 (0.19)	6.86 (0.15)
CD	7.13 (0.30)	7.67 (0.26)	7.44 (0.20)

Note: Standard errors in parenthesis

Result 1

- **a:** *Principals of both genders earn more when delegation is compulsory.*

⁵From now on when we present the average earnings of subjects we will not include the £5 show up fee and the £2 for filling the questionnaires.

⁶From now on we use Mann-Whitney non-parametric tests to study the hypothesis that two independent samples are from populations with the same distribution.

⁷Bottino et al. (2016), find no significant differences in principals earnings in BS, still they find that males earn quantitatively less than females (males earn 7.53 and females 7.83).

⁸Using a Mann Whitney test to compare the distributions of male and female principals earnings: CD ($z=-1.41$, $p=0.16$), ED-D ($z=0.851$, $p=0.39$), ED-N ($z=0.345$, $p=0.73$).

- **b:** *There are no gender differences in principals earnings in endogenous or compulsory delegation, however, male principals earn less in a standard dictator game*

Hypothesis 2 predicts that males will allocate lower amounts when they are selected as agents. In fact, in Table 5.3 we observe that male agents re-allocate lower amounts to recipients in CD ($z=6.22$, $p=0.00$), but we do not observe gender differences in agents behaviour in ED-D ($z=1.12$, $p=0.26$).⁹

Table 5.3: Amounts allocated by agents and principals (by treatment and gender)

	Male principals	Female principals	Male agent	Female agent
BS	4.21 (0.18)	3.03 (0.18)	—	—
ED-D	—	—	4.01 (0.32)	3.56 (0.18)
ED-N	3.06 (0.26)	3.29 (0.21)	—	—
CD	—	—	1.64 (0.21)	3.83 (0.31)

Note: Standard errors in parenthesis

Hypothesis 2 also predicts that male agents will be selected more often as a result of them redistributing less in both delegation treatments. To check if male agents are selected more often we analyse the amount of times principals select male and female agents under CD and ED-D.

⁹See Figures 5.1 and 5.2 in Appendix A for a graphical comparison of the amounts redistributed by each type of players in BS, CD and ED treatments.

Figure 5.1 shows the proportion of principals choosing each genders' agents by round in CD treatment. Under CD male agents allocate lower amounts to recipients. As predicted by previous literature, male agents were selected more often (61%) than female agents (39%) in CD. The tendency of choosing more male agents is similar along all the periods.

Figure 5.1: Proportion of male and female agents selected by principals in CD

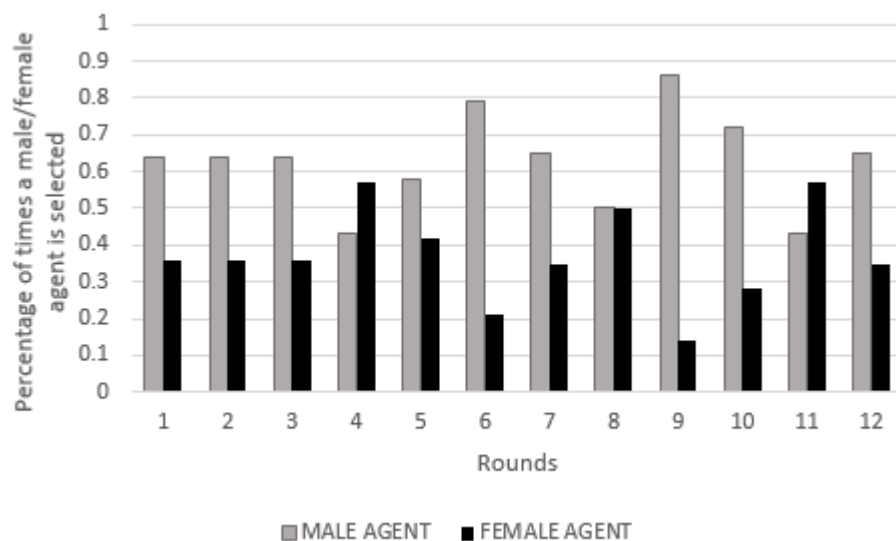
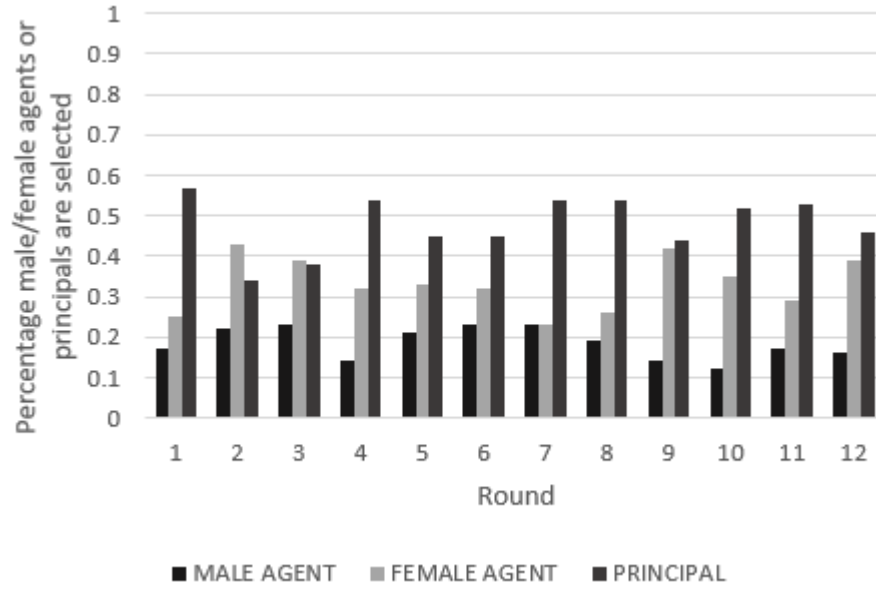


Figure 5.2 illustrates the proportion of principals choosing not to delegate in ED and the proportion of male and female agents selected by those principals who chose to delegate. In this treatment male agents were not allocating lower amounts than their female counterparts. As a consequence, male agents are selected on average the 18% of the cases while females are chosen on the 32%.

Figure 5.2: Proportion of decisions made by principals and male and female agents in ED



Agent selection by gender of the principal still results in male agents being selected more often in CD by both male (69%) and female (59%) principals. By contrast, in ED male principals delegate in male agents the 18% of the decisions (male principals do not delegate in the 52% of the situations), while female principals choose male agents in the 19% of the cases (and do not delegate in the 49%). Hypothesis 2 does fulfil in CD but not in ED where male agents are not selected more often.

To study the relationship between hiring an agent and the amounts allocated by this agent we perform two OLS and probit regressions analysing principals decisions (Table 5.4). Both OLS linear Models (1 and 2) have the same dependent variable: the amount allocated to recipients. In Model 1 the data analysed is CD and the explanatory variable is the gender of the agent making the decision on each round. Model 2 uses the ED data and the explanatory variables are the gender of the decision maker (only when the se-

lected decision maker is an agent) and the round.¹⁰ Models 3 and 4 present two probit models with the same dependent variable with value 1 if the principal decides to switch the decision maker and 0 otherwise. In Model 3 the data analysed is CD and in Model 4 ED. The independent variables in both models are the gender of the decision maker (1 if is a male), the round and the amount given in the round prior to deciding to switch the decision maker.

Table 5.4: Switching in round t based in round t-1

	OLS		Probit	
	Model 1	Model 2	Model 3	Model 4
Gender of the decision maker	-2.03*** (0.08)	0.52 (0.35)	-0.19** (0.20)	-0.30*** (0.10)
Round	-0.19*** (0.05)	0.12*** (0.02)	0.03 (0.1)	-0.01 (0.03)
Amount given in previous rounds	—	—	0.18*** (0.03)	0.07*** (0.00)
Constant	4.90*** (0.44)	4.39*** (0.05)	-0.96*** (0.27)	0.05*** (0.13)
Observations	187 (17)	323 (59)	187 (17)	649 (59)
Log likelihood	-99.46	-110.57	-395.88	-395.84
Prob > F	0.00	0.02	0.00	0.00

, where the number in parenthesis represent the standard errors for each coefficient. *** p<0.01, ** p<0.05, * p<0.1

¹⁰Round 12 is excluded from regression to avoid end game effect.

In Model 1 the explanatory variable gender of the decision maker is negative and significant at a 1% level. This indicates that if the selected agent is a male, the amount reallocated to the recipient will be lower in the CD treatment. In addition, the coefficient associated to the variable round is also significant and negative. Lower allocated amounts are expected as rounds advance. In Model 2 the coefficient associated with the gender of the decision maker (taking into account only selected agents) is not significant. Meanwhile, the coefficient associated to the round is significant, negative and has a similar size as in Model 1. Therefore, in CD male agents reallocate lower amounts to recipients across rounds. In ED we do not observe differences in amounts shared by the agents gender.

In Model 3 we find that when the principal is a male the switching likelihood decreases in 0.19. This value is significant. The amount given in the previous round has a positive and highly significant coefficient (0.18). The round is not significant. In Model 4 the coefficient associated to the gender of the decision maker is significant and negative (-0.30). If the principal is a male less switching of decisions are predicted. The amount given in the previous round is significant and positive (0.07) and the round is not significant. Therefore, principals switch more often when amounts reallocated are higher in CD than in ED. Also, when the decision maker is a male the likelihood of switching agents is lower in both ED and CD. Female principals switch decision makers more in both treatments.

Following Bottino et al. (2016) we use panel data regression models to analyse the selection process from agents perspective. We use a generalized least squares regressions with cluster-robust standard errors estimation on agents data.¹¹ In both Models 5 (CD) and 6 (ED) the dependent variable is the average allocation made by agents in a period. The set of independent variables include the profit of the agent that period (the level of success of the agent), the gender of the agent and the round they are in.

¹¹The results are consistent using a clustered OLS estimation with robust standard errors.

Table 5.5: Agents

	CD	ED
	Model 5	Model 6
Profit	-4.15*** (1.28)	0.17 (0.48)
Male	2.24*** (0.40)	-0.03 (0.32)
Round	-0.05 (0.39)	-0.08** (0.44)
Constant	4.20*** (0.48)	4.21*** (0.32)
Observed principals	10	34
Prob > χ^2	0.00	0.22

,where the number in parenthesis represent the standard errors for each coefficient. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Model 5 indicates that in CD the profit made by agents and the agent being a male considerably reduces the amounts allocated to recipients independent of the round. This confirms that in CD male agents allocate lower amounts to recipients and also that their success depends on lower amounts redistributed. In Model 6 only the coefficient associated to the round is significant and negative. Therefore in ED the level of success of the agent and its gender do not affect the allocation decision made by the agent.

- **Result 2:** *Male agents only redistribute lower amounts in CD, as a result they are selected more often than females in this treatment. In ED, male agents do not redistribute lower amounts and as a result, they are not more successful than females.*

Summing up, when delegation is compulsory principals earn more as they select those agents who redistribute lower amounts to recipients, this successful agents tend to be males. By contrast, when delegation is optional,

amounts redistributed to recipients are higher and gender bias in agents selection disappears.

Conclusion

We use a modified dictator game (Hamman et al. 2010) to explore gender biases in compulsory and endogenous delegation. We find that male and female principals earn similar amounts when they are in the same delegation treatment (in line with Bottino et al. 2016). In addition, principals of both genders earn significantly more when delegation is endogenous (and in the standard dictator game) compared to forced delegation. This suggests that there are no gender biases in the top level position where male and females behave equally.

We also find that in compulsory delegation both female and male principals select the agent who shows less fairness concerns. The agent allocating lower amounts to recipients is usually a male and therefore male agents are selected more often. Therefore, compulsory delegation generates gender discrimination in agents selection. This tendency to choose the agent who shares less with recipients is weaker when delegation is optional. Consequently male and female agents reallocate similar amounts to recipients as none of them is competing through lower reallocated amounts. Endogenous delegation prevents gender discrimination in the selection of agents.

Male agents success in compulsory delegation could be linked to male agents being more efficient in competitive environments (Gneezy et al., 2003; Nierdele and Vesterlund, 2007). In compulsory delegation the relationship between allocating lower amounts and being selected more often is clear for agents while in endogenous delegation the best way to compete is not so clear.¹²

¹²When delegation is optional agents seem to stop competing by giving lower amounts to recipients as they did in compulsory delegation. This difference is not driven by informational differences between treatments as we run an extra endogenous delegation treatment that was informationally closer to compulsory delegation, by not mentioning the alternative to delegating. This extra treatment gives us exactly the same results as the original endogenous delegation treatment.

Another explanation could be in Gino et al. (2015) who find that men and women view professional advancement differently. Women value high level positions less as they see more negative aspects associated with them. Delegation is a tool to shape the future hierarchical structure of organizations. Agents who perform well when delegated will continue receiving more responsibilities and ascending. Therefore, following the findings of Gino et al. (2015) women agents have different objectives than male agents who value more being selected by principals and therefore are willing to allocate lower amounts to recipients. Females on the other hand are less concerned with being selected more often (and ascending) and more concerned about fairness in the redistribution of the endowment.

Concerning future research, the dictator game is a very specific environment and it would be worth extending this analysis to other types of settings (i.e. a setting involving effort from participants or team work) and games (i.e. ultimatum game). Such contexts could help to understand better the relationship between delegation and gender differences.

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Appendix A

Table 5.1: Answers to the questionnaire

	BS		CD			ED		
Player	P	R	P	R	A	P	R	A
I feel involved in the experiment	1.85 (1.55M/ /2F)	-0.78 (-1.11M/ /1.3F)	0.17 (-0.6M/ /0.6F)	-0.82 (-0.1M/ /-1.6F)	0.88 (1.6M /1.3F)	1.30 (1.3M/ -0.8F)	-0.43 (-0.3M/ 0.5F)	-0.27 (-0.2M/)
Profit was relevant	1.11 (1.7M/ /1.3F)	0.68 (1.2M/ /0.2F)	1.57 (2M/ /1.2F)	0.05 (0.3M/ /0.2F)	0 (0.8M/ /1.2F)	1.22 (1.4M/ /0.1F)	0.91 (0.4M/ /0.3F)	0.03 (0.3M/)
I consider my behaviour acceptable	2.25 (2.1M /2.3F)	2.68 (2.4M/ 2.9F)	2.64 (2.7M/ 2.6F)	1.82 (2M/ 1.6F)	2.87 (2.8M 2.8F)	1.66 (1.5M/ 1.7F)	2.32 (2.22M/ 2.1F)	1.64 (1.55M/ /1.7F)
Players P had an acceptable behaviour	—	0.10 (-0.3M/ /0.5F)	—	-0.23 (-0.3M/ /-0.1F)	0.70 (1.2M/ 0.2F)	—	0.56 (0.5M/ /0.5F)	0.44 (0.3M/ /0.5F)
Players A had an acceptable behaviour	—	—	1.86 (1.8M/ /1.1F)	—	—	1.05 (0.7M/ /1.1F)	—	—
I am ready to take risks	1.46 (1M/ /1.7F)	1.73 (1.6M/ 1.8F)	1.94 (2.1M /1.8F)	1.76 (1.7M/ 1.8F)	2.30 (2.2M/ 2.4F)	1.35 (1.3M/ 1.3F)	1.89 (1.8M /1.8F)	1.87 (1.9M /1.7F)

Note: Average values of the answers to a seven item scale questionnaire, from strongly disagree (-3) to strongly agree (+3) with a neutral option (0). Average values for males (M) and females (F) in parenthesis.

Figure 5.1: Amounts allocated to recipients by male and female principals in BS and ED-N

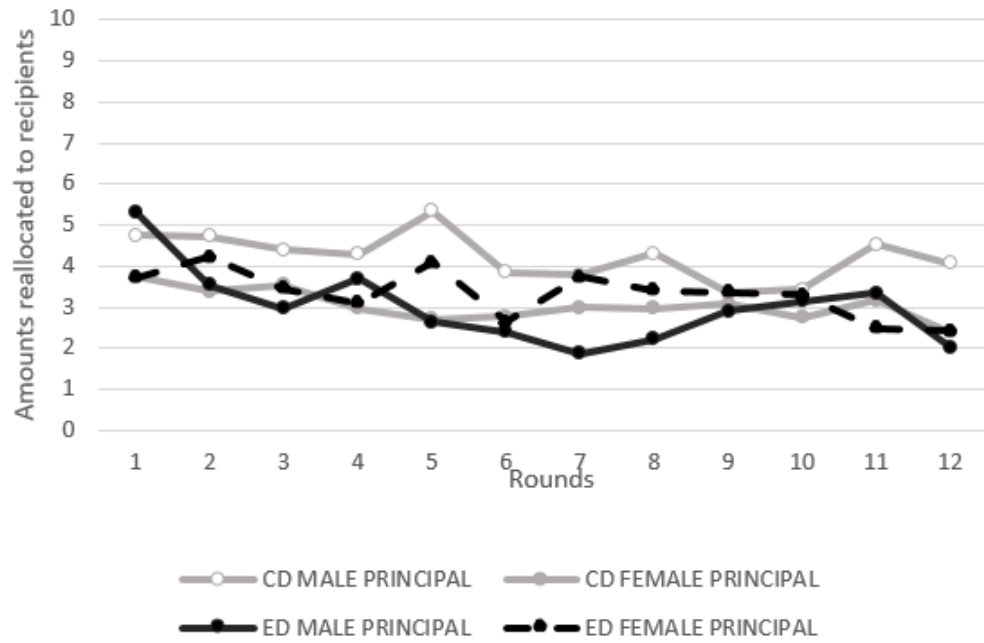


Figure 5.2: Amounts allocated to recipients by male and female agents in CD and ED-D

